OSU WPL # 25 (1981) 67-106.

Some Further Problems for Upside-Down Phonology

Donald G. Churma

Leben and Robinson 1977 (henceforth L&R) have presented a theory of phonology in which phonological rules function, not to derive surface pronunciations from underlying representations as in standard generative theories, but 'to permit the morphology to relate words that superficially are phonetically dissimilar' (L&R:1). This is accomplished by 'undoing' the rules of standard treatments, subject to certain constraints specified by L&R. This theory, which is termed 'upside-down' phonology (hereafter UDP), is claimed to have attributes which make it significantly more attractive than other currently proposed theories. In this paper, it will be argued that, from a synchronic perspective, UDP is both too weak, in that it either does not allow words which should be related to be related or is forced to treat an apparently uniform phenomenon as two or more separate phenomena, and too strong, since it allows words to be related which should not be. It will also be argued that UDP, at least in a form which can handle certain synchronic facts which it could not otherwise account for, does not have many of the diachronic advantages claimed for it. Finally, it will be suggested that psycholinguistic considerations present significant difficulties. Before proceeding to these arguments (sections 3, 4 and 5), however, I will first present an outline of the theory in its various manifestations (section 1), and then give a brief discussion of some previous criticisms of UDP, especially the long critique given in Janda 1980 (section 2).

1. The theory.

According to L&R (p. 1), 'the central function of phonological rules is to answer this question: "Given two words whose pronunciation and meaning are in the lexicon, are A and B related morphologically"?' Thus, in the case of their example sane/sanity, the phonological rules apply so as to allow these words to be related by morphological rule (1) (cf. L&R:1).

(1) Word-Formation Rule: -ity Attachment

$$[\text{ADJ} - \text{ətiy}]_{\text{N}}$$

By altering the phonological shape of the stem, the phonological rules eventually allow sanity to be parsed as sane plus -ity. L&R's conventions on rule aplication (p. 2) are given in (2), and a sample derivation relating sane and sanity in (3).

(2) Conventions on rule application.

a. If, in a conventional generative treatment, a form is derived by three rules A, B, C, applying in that order, they apply in our account in the reverse order, C, B, A, except as provided by (2c) below.

b. A rule of the form X → [-F]/Y_Z is undone by replacing [-F] with [+F] on segment X in the environment Y_Z. Analogously, a rule of the form Ø → X/Y_Z is undone by deleting X from the context Y_Z.

c. A rule is blocked if undoing it would not increase the compatibility of forms A and B with respect to Word-Formation Rule R.

(L&R:2)

(3) Sample upside-down derivation of sane/sanity (adpated from L&R:3):

	Word A	Word B	Morphology
LEXICAL FORMS	[sēyn]$_A$	[sænətīy]$_N$	[ADJ-ətīy]$_N$
a. Vowel Shift	[sǣyn]$_A$	———	———
b. Diphthongization	[sǣn]$_A$	[sænətī]$_N$	[ADJ-ətī]$_N$
c. Laxing	———	[sǣnətī]$_N$	———

Once stage (3c) is reached, the stems are phonologically non-distinct, and sane and sanity are recognizable as being related by rule 1.

As noted by Janda, condition (2c) is an extremely important part of this theory, especially since it is this condition which is mainly responsible for the ability of UDP to relate pairs which require exception features in standard theories 'without resorting to ad-hoc means' (L&R:4). Unfortunately, as stated, it is somewhat vague in that neither 'compatibility', as Janda has pointed out, nor 'form' has been explicitly defined.

To remedy the first vagueness problem, Janda (pp. 8-20) suggests two possibilities. The first would require (p. 8) that to increase compatibility 'would be either to increase the number of shared segments, either by changing one pre-existing segment to match another pre-existing segment, or by adding a new segment to match a pre-existing segment and/or to decrease the number of unshared segments, by deleting a pre-existing segment that has no match in the other form, in corresponding positions in the two forms A and B'. This interpretation is pretty clearly not what L&R intended, since several of their derivations violate it (cf., for example, the relating of [jɔ̄r] and [järig] on p. 13), and I myself had never even thought of this possibility until I heard Janda 1977. Since Janda also gives another example which requires violation of condition (2c) if compatibility is interpreted in this way, such an interpretation seems clearly undesirable.

Janda's other suggestion in this respect (p. 14) seems much closer to what L&R intended. Here compatibility would be defined 'in such a way as to allow individual feature-values to remain--or even to become--different so long as there is a rise in the overall number of shared feature-values'. Janda has some objections to using this '"weak" definition' (p. 15). First, it is 'intuitively repugnant' in that it entails that the Finnish words 'virsi and virren are more compatible because their [s]~[r] contrast has been altered to an [s]~[t] contrast'. Secondly, 'the only real reason for undoing' a rule which increases compatibility in this sense is to allow a later rule to be undone, thus requiring

considerable 'global' power if derivations in accord with this interpretation of compatibility are allowed. It is also (pp. 16, 18) 'ad hoc,' but must be adopted anyway because of the empirical inadequacies of the 'strong' version (p. 18). All that I can say in this respect is that I do not share Janda's intuitions about the repugnance of this interpretation--[t] does indeed appear to me to be more like [s] than [r] is--and that I therefore do not agree about the 'real reason for undoing' rules in cases of this nature. I cannot agree, either, that such an interpretation makes the definition ad hoc, although I am not entirely sure that I understand what Janda intends by this term here. I will thus assume 'compatibility' to be defined in terms of shared feature specifications, rather than whole segments, in the remainder of this paper.[1]

It is also somewhat unclear what the term 'form' in condition (2c) should be taken to refer to; it could be either 'word' or 'morpheme'. L&R's elaborations on this point in the course of their illustrations of this condition do not seem to be of much help in resolving the vagueness; they in fact suggest both interpretations at different points in their discussion. Thus, they note (p. 3) that 'once we get to stage (3c), we see that words A and B have representations of the stem san- that are non-distinct...' (emphasis added in all of these citations). However, two sentences later, they state that 'convention (2c) enjoins us from bothering with [further rules--DGC], since further applications would not increase the compatibility of words A and B with respect to Rule 1'. On p. 4, the implication is again that it is morphemes, not words, which are involved: 'we have proposed that rules are blocked if they do not increase the similarity between two allomorphs in a derivation'. Their examples also appear to point in contradictory directions with respect to this issue; thus we find (pp. 8-9) that rules which affect affixes are undone in relating Ketill and Kötlum, but not in the case of jaki and jökli, which are claimed not to be directly relatable by L&R. But they are in fact relatable, as long as it is permissible to undo rules which affect affixes. According to Anderson (1974), whose analysis L&R follow, jökli 'glacier' (dat. sg.) is underlyingly /jak+ul+e/ (where -ul is some kind of stem-forming suffix), and the surface form is derived via rules of syncope (which deletes vowels in contexts which are not relevant to this discussion) and u-umlaut (whereby a becomes ö when followed in the next syllable by u), as well as the rule which accounts for the change of final e to i and is irrelevant here. It should be clear that undoing syncope on jökli increases its compatibility with jaki with respect to the -ul- suffixation rule (and the rules which account for inflectional affixes). And once it has been undone, u-umlaut can be undone, giving jak+ul+i, and the two forms can thus be identified as being morphologically related according to the WFRs in question. It is thus not at all clear what L&R intend in this respect. However, since there appear to be a significant number of cases which would require the 'word' interpretation (including many of those discussed below), it seems clear that this interpretation must be adopted.[2]

One further point deserves mention in this respect. As L&R have noted (p. 10), morphologically related words do not always involve only a single WFR, contrary to the implications of condition (2c). Such a situation is not restricted to 'polysynthetic language(s) with many layers of morphology built into a single word' (L&R:10), but will also occur (at least) whenever the words involved belong to an inflectional paradigm and have

phonologically non-null inflectional endings. While in most cases examined here the required extension of condition (2c) seems intuitively clear, in some cases the intended UDP interpretation is not at all obvious (cf. section 3.1.4 for further discussion).

Two refinements on the conditions should also be mentioned here. The first is somewhat trivial; Leben (1977 a, b, c, 1979) has expanded condition (2b) to include the obvious specification of how to undo a deletion rule--the Ø output is replaced by the input. The modification suggested by Robinson 1977 is not at all trivial, however, and in fact entails a considerable shift in the class of possible phonologies from the UDP perspective. Robinson never explicitly formulates his revision (which apparently is intended to replace, at least in part, both conditions (2b) and (2c)), but what he intends seems clear enough. I repeat below in (4) Janda's (pp.25-6) reconstruction of Robinson's revision:

(4) i. Check whether a given morphological rule M can apply to relate Words A and B. If it can, the derivation stops; if not, then--

ii. Attempt to undo phonological rules, in the following way:

a. Check whether either or both of Words A and B is a possible output of the first relevant phonological rule (e.g., has on some segment/s in the correct environment, feature-values non-distinct from those present--explicitly or implicitly--in the structural change of the rule). If not, proceed to the next phonological rule and repeat this step; if so, then--

b. Substitute variable feature-values for the specific values of exactly those features on the segment identified as a possible output of the rule in question, which explicitly appear in the structural change of that rule.

c. Check whether the segment in corresponding position in the other form bears specific values for these same features. If not, return to (i) above; if so, then--

d. Substitute these specific values for the variable values of the features in the segment changed by (b). Then return to (i).

The intended effect of this revision, if it is not already clear from this statement, should become so when it is applied to actual examples below. But let us turn now to an examination of some previous critiques of UDP.

2. Some problems and some non-problems for UDP.

This section will be devoted to an examination of previous criticisms of UDP, especially that of Janda 1980. Some of the specific objections raised are found not to be serious problems for UDP, while others appear to be genuinely problematic for the theory. It is these latter objections which I will consider first.

2.1. Problems.

The first problem, discussed by Janda (pp. 16-7, 32-4), concerns an aspect of UDP not yet brought up. L&R, in connection with the alleged impossibility of directly relating jaki and jökli discussed above, suggest that it is also possible to relate forms indirectly by relating both of them to a third form (L&R:10). There is no question that such a provision will allow a considerable number of forms to be correctly related which could not otherwise be. But there is some question whether it can take care of all of the morphologically related words which cannot be shown directly to be so related. Particularly troublesome would be (Janda 1980: 17) 'a defective paradigm--one either riddled with accidental gaps, or adulterated by suppletion', as well as, I would add, paradigms which inherently have only two members (e.g., singular/plural). Although Janda has given no examples which would indicate that this is an actual empirical problem (and not just a potential problem), it seems clear that UDP will eventually have to deal with cases like this--there is no reason at all to believe there will always be a third form which 'comes to the rescue' when two forms cannot be directly related (cf. also the discussion in section 3.1.4 below). It should also be pointed out that allowing for such a possibility would make for a situation in which, as L&R put it in tentatively rejecting an alternative to condition (2a) (p. 8), 'the parsing procedure provided by the grammar would be much less determinate', since there is no guarantee that the 'correct' third form will be chosen on the first try.

This brings up a somewhat related problem, that of how to tell, in a determinate fashion, when two forms are not morphologically related (cf. Janda 1980:36-7). The problem is that whenever two semantically related words (e.g., depart/left) are not also morphologically related, it is possible that several morphological rules must be tried out in an upside-down attempt to relate the two words. This would be the case in instances where a language has different ways of performing a morphological process, depending on the (arbitrary) lexical class to which the lexical items in question belong, such as the rules which form past tense in English.[3] In the case of depart/left, then, all of the past tense formation rules would have to be checked, attempting with each such rule to relate the two words by undoing the phonological rules of English. Clearly, in this case it would take some doing to show that depart and left are in fact not related by any of the past tense formation rules of English.

Janda also notes (p. 20) that, at least in the case of a hypothetical example he constructs (pp. 18-9), UDP 'must...have global phonological rules that look farther ahead than at their own output' (and not simply at their own output, as required by condition (2c)). This is true not only of this hypothetical example, moreover, but also of a real example which will be discussed below (section 3.1.1).

Another problem noted by Janda (pp. 41-3) concerns condition (2b) when certain kinds of neutralization rules are involved. The rule of English which reduces unstressed lax vowels to schwa, for example, creates severe difficulties for this condition if it is extended in the natural way to cover (rightside-up) rules which alter more than one feature value of the input (i.e., replace each feature value mentioned in the output by the opposite plus/minus value). Since schwa is, among other things, [-high, -low], this rule, when undone according to this procedure, would result in a segment which is [+high, +low]--a physical impossibility in the

system of Chomsky and Halle 1968 (as well as most others)--and it is certainly undesirable from the standpoint of UDP in any event, even if it were a physical possibility. This appears not to create a serious difficulty for the theory, however, since it is a trivial matter to revise condition (2b) so that rules are undone in a quite literal fashion--the output of this rightside-up rule is replaced by its input, whether it is a deletion, insertion, or feature-changing rule. The problematic aspects of L&R's original condition (2b) uncovered by Janda can thus be seen to be fairly readily remedied.[4]

Janda has also pointed out (pp. 46-8) an apparent inconsistency in L&R's claims about the 'complicatedness' of forms. On the one hand, they indicate (L&R:3) that obesity is not 'complicated', since it 'can be parsed at the surface without any consultation of the phonological rules', despite its 'exceptionality...with respect to laxing'. On the other, in the context of a discussion of historical change from the perspective of UDP, they claim (p. 19) that 'apparent underapplication of a rule is resolved by changing the deviant item to support the threatened rule'. That is, obesity is predicted to change (and thus introduce allomorphy), despite the fact that it is not 'complicated'. Even if the claimed uncomplicatedness of obesity and its susceptibility to change can be reconciled, there appears to be a problem concerning the diachronic predictions made here. The change in fact appears to be proceeding in just the opposite direction--the pronunciation [ōwbĭysətĭy] appears to be an innovative one, replacing older [ōwbesətĭy] (which is the only pronunciation given in the OED, and a variant pronunciation in Webster's Third). That is, the underapplication of laxing is being introduced, and not eliminated to 'support the threatened' laxing. (Thus (Janda, p. 48) 'it is...incorrect to claim that there is nothing complicated about a form like obesity and that [standard theory] errs in marking it as exceptional, and thus strongly predicting a change in its pronunciation'--the change, again, is apparently in the opposite direction). Such cases are not at all rare, and in fact the theory of 'suppletive' lexical representations of Hudson 1974, 1980, has been specifically designed to deal with them. But there are cases which are in accord with L&R's prediction, such as that of Swiss German umlaut (L&R:18) which is supported by a change in pronunciation, so Hudson's theory has its problems as well. In fact, I know of no theory which makes all the right predictions in cases like these, so this problem is not unique to UDP. But such cases do seem to make it less 'clear' (L&R:19) 'that the notion of opacity as a motivation for change finds a much more comfortable home in our theory than in the standard framework'.

The final objection rasied by Janda (pp. 46-9) to be discussed in this subsection concerns the apparent failure of one of the diachronic predictions of the theory. L&R state (p. 19), as noted above, that 'apparent underapplication of a rule is resolved by changing the deviant items to support the threatened rule', but Janda adduces an example from Yawelmani where this seems not to be the case. It involves the notorious rule of vowel harmony (see Janda for references), with respect particularly to the passive-aorist suffix. This suffix appears phonetically as -it in most cases, but after stems with -u- and, crucially, some stems with -o:-, it shows up as -ut. Since UDP does not have to worry about exceptions to rules (cf. L&R:3-5, and below), the obvious way of accounting for the suffix alternation would be via a vowel harmony rule which rounds

and backs suffixal i when it follows a rounded vowel. Such a rule will allow the variant forms of this suffix to be related by simply undoing this rule whenever an -ut form is being compared with an -it form, and in the other cases, they can be directly related without undoing any rules. But, Janda points out, given such a synchronic analysis, there is 'apparent under-application' of the vowel harmony rule (it does not apply after some mid vowel stems), and UDP therefore predicts an increase in mid vowel stems which trigger harmony. But the actual change, given the rather meager amount of evidence which bears on this issue, appears to be heading in the opposite direction--there is now a mid vowel stem which takes a suffixal -i- as well as the earlier -u-. The only way out of this problem, Janda maintains, is to adopt a diacritic analysis in which only high rounded vowels trigger harmony. Such an analysis would undoubtedly not be acceptable to proponents of UDP. But there may in fact be another way out for UDP in this case: the vowel harmony rule could be restricted so that it applies only after high rounded vowels, and -u- suffixes after mid stem vowels could be related to the corresponding -i- suffixes not directly but via a third form which contains an -u- suffix after a high stem vowel. The two -u- suffixes would match without undoing any rules, and the one following the high suffix vowel could be related to the -i- suffix by a straightforward undoing of the restricted vowel harmony rule. It is not clear to me whether such an alternative would be acceptable to the proponents of UDP, since they nowhere to my knowledge discuss attempts to establish the morphological identity of affixal allomorphs (this whole issue appears to merit further inquiry in this respect). Robinson (1980:132) implies that the use of a third form might be limited to 'derivationally related forms', and would not be allowed for 'comparing the members of a single paradigm', and Leben (personal communication) has made comments which suggest that he might subscribe to a similar view. The case at hand does not really fit into either of these categories, since the suffix in question is not a derivational one and the forms involved are not 'members of a single paradigm'. I know of no discussion in the literature which is relevant to the issue of the categories with which cases such as this are most closely related (assuming this is a legitimate question to ask), although it seems to me that affixes which show allomorphy are much less subject to pressure for change than stems within a paradigm. It is thus unclear whether the suffixal allomorphs in question could (or should) be related via a third form.

2.2. Non-problems.

While I agree with Janda that the above criticisms are, at least to some extent, genuine problems for UDP, this is not the case with respect to all of his criticisms. I will devote this subsection to a brief outline of the reasons for my disagreement; in so doing, I hope to clarify the real issues as far as an evaluation of UDP (or any other theory) is concerned, and thus to indicate why I feel that UDP deserves sufficiently serious consideration as a theory of phonology that it requires the further criticisms given below.

Janda's first criticism of UDP is that (p. 7) 'all rules in UDP must crucially be global', since condition (2c) requires that the potential output of a rule be examined before it can be determined whether or not it is applicable. This is quite true, and it is also true that rules in UDP are inherently transderivational (cf. Lakoff 1973), since their outputs must be compared to outputs in different derivations. It is not

at all clear to me why Janda brings up this point, since he claims (p. 8) that 'such "local" globality does not increase the class of possible grammars..., and so it is hard to characterize it as objectionable...'. He does not provide any arguments to support his claim about the class of possible grammars entailed by UDP, and since similarly glib statements concerning the relative generative capacity of various revisions of UDP can be found throughout, it is worth a brief digression to say a few words about this question.

Globality (and transderivationality) have gotten a lot of bad press recently (cf., for example, Baker and Brame 1972, Langendoen 1975), and it is tempting to believe that such characteristics are inherently bad. But the reason that they were held to be objectionable had nothing to do with their inherent qualities--the arguments go that they (unacceptably) increase the size of the class of possible natural languages. Such an increase, or lack thereof, must be demonstrated, and cannot be established by fiat. It is not always easy to provide such a demonstration, and if one cannot provide one, then the only rational thing to say in such a situation is that the relative power of the theories in question is not known. One should be quite clear, moreover, on what is required for a demonstration that one theory is more powerful (i.e., less restrictive) than another. Since linguistic theories typically generate an infinite number of possible languages, it must be shown not only that there are languages which the putatively more powerful theory can generate and the other cannot, but also that there are no languages which are generable by the latter and not by the former.[5] In many cases, it will not be possible to do so, and the two theories may well be incommensurable (incomparable) in the mathematical sense. But they need not be incomparable from a linguistic point of view. As long as the theories make different claims about what the (infinite) class of possible natural languages is, they can be assessed on the basis of the correctness of their claims. Thus, in the case of incommensurable theories, if it can be shown that one of the theories can generate an impossible natural language, or cannot generate a possible (preferably actual!) language, while this is not the case for the other theory, then the latter is to be preferred to the former. It should also be pointed out that in cases where one theory can in fact be shown to be more powerful than another, it must also be shown that such an increase in power is unacceptable in that the additional languages generable are not possible ones.[6] Janda has nowhere, as far as I can tell, addressed this issue directly. It will thus be the main focus of my own criticism of UDP from a synchronic perspective--I will argue that UDP is both too weak in that there are possible languages which UDP cannot generate, and too strong in that there are also impossible languages which UDP can generate.

Janda also fails to address this issue when he claims (p. 29) that the revised procedure for rule application suggested in Robinson 1977 ((4) above) has 'excessive power'. The only possibly relevant attempted justification for this claim (an irrelevant one will be discussed immediately below) is that this procedure 'reduces the derivation of Finnish virsi/virren to a single step...'. Since these two forms are clearly morphologically related, as Janda (pp. 10-1) agrees, I cannot see how this fact can show that UDP can generate impossible languages. This procedure, like L&R's original procedure ((2) above), does in fact appear to be too strong, as argued in section 3.2, but nothing in Janda's exposition would lead one to believe that this is the case.

A seemingly relevant observation by Janda (p. 29) in this respect is that 'the phonological rules in an UDP incorporating [4]...are in danger of relating scene to sanity. But this is only possible if it is not the case that, as Janda puts it (p. 30), 'one stimulates that a semantic comparison of some kind...is performed before any morphological or phonological rules in a derivation are undone...'. Such a stipulation is rejected by Janda, since 'the fact that semantics, and not phonological rules (by virtue of not being able to apply), is what is necessary to prevent the relating of,e.g.,scene and sanity...must certainly count as a further strike against this version of UDP. But surely it is in fact semantic factors which are responsible for the knowledge that speakers of English have that scene and sanity are not morphologically related, and not phonological ones. And it is undoubtedly the case that whenever two words are semantically unrelated (at least, if they are as unrelated as these two), speakers of the language in question will judge them to be morphologically unrelated as well, and precisely because they are not related semantically. As the example cost/caustic, given by Leben 1979:185 (and cited by Janda), clearly shows, even phonological identity of what is putatively the same stem is not enough to establish morphological relatedness, and the only way of preventing these two words from being related is 'to equip...morphological rules with semantic characterizations that must be satisfied by words related by such rules' (Leben 1979:185). Neither can I see why Janda apparently feels (p. 29) that it would be desirable that 'semantics...not come into play in derivations in UDP until morphological rules can actually apply...'. Speakers are not even tempted to suspect that Janda's microorganism and lick are related, because of their semantic disparity, and so it is entirely appropriate that the grammar, which is a model of speakers' knowledge, not also be 'tempted' to undo Vowel Shift on the first vowel in this pair, again because of their semantic disparity. Far from being a defect of UDP, attributing the morphological unrelatednesss in such cases to semantic factors seems clearly to be desirable, since it is just such factors which are behind speakers' judgments about morphological relatedness. In fact, any theory which claimed that scene and sanity were not related for phonological reasons would surely merit a good bit of scepticism.

McCawley 1979:295 raises what appears to be a similar objection (his example in moth/mother). It is thus subject to the same kinds of criticisms as Janda's objection, although I agree with McCawley that it might be worthwhile 'to consider the possibility that different morphemic identities may have different psychological status', and should thus have formally distinct representations in a theory of morphology.

Janda also discusses (pp. 33-5) what he feels to be a problem which is brought about by the possibility of relating two forms via a third form--this possibility (p. 35) 'greatly multiplies the number of incorrect derivations produced by UDP.' Thus, in the case of the Finnish triple virsi/virren/virsia, 'UDP will render the incorrect verdict that only the first and the last of these three forms are morphologically related' (if Janda's 'strong' version of condition (2c) is used). But there is no reason to believe that verdicts about unrelatedness in UDP should be arrived at as quickly as the above statement implies. A perfectly acceptable definition of unrelatedness is that there be no possibility of relating the forms in question directly and that there be no other form which serves to relate them indirectly. Under this interpretation, no verdict at all

about relatedness is reached until either a match is found (in which case the forms are morphologically related) or all (semantically related) words have been checked as possible 'third forms' and no match has been found (they are not related). There is thus no obvious problem related to (p. 34) 'generating all and only the correct output [sic]' which is due solely to the 'third form' possibility, and the only reasonable way of going about determining whether there is in fact a problem is the empirical one (cf. the discussion of globality above): does allowing 'third form' derivations correctly characterize the class of possible natural languages, or does it not? This question is simply not addressed when one flatly claims, as Janda does (p. 34), that 'for UDP to be an interesting theory of phonology, an unsuccessful derivation must mean that the forms in it are (all) morphologically unrelated...'.

In a similar vein, Janda (pp. 22-4) criticizes the 'unconstrained' character of the proposal to abandon condition (2c) and to allow rules to be undone optionally, with an unrelatedness verdict given only when all possible combinations of actual applications of rules have been tried and found not to produce a match. He even claims (p. 22) that such a procedure 'effectively immunizes UDP against ever being faced with a counter-example'. In this case, Janda does attempt (p. 22) to give some justification for this claim, but it is not at all clear to me that this attempt succeeds. For example, as long as there is (p. 22) 'a linear list of phonological rules', then if this linear ordering entails that the undoing of one of the rules counter-feeds (cf. Newton 1971, Koutsoudas, Sanders and Noll 1974) another, and the counter-fed rule must be undone to relate some pair of forms, then the forms will not be relatable by this procedure--the environment of the counter-fed rule will never be met, since the counter-feeding rule cannot be undone until after the counter-fed rule has been. If such a situation should be uncovered, it would in fact be a counterexample with respect to a version of UDP which incorporates such a procedure. It should be pointed out in this respect that this kind of revision to the theory does indeed appear to make the revised version more powerful than that proposed by L&R, since the optionality of undoing rules appears to give the same effect as does condition (2c) in blocking 'bad' undoings, and the possibility of undoing rules even when compatibility is not increased allows for languages which could not be generated if condition (2c) is maintained. But in order for this to count as a defect of the theory, it must be shown that this increase in power is undesirable, i.e., that it permits the generation of impossible languages.

It is interesting in this respect that the kind of evidence mentioned above would not be a counterexample to the proposal considered (and tentatively rejected) by L&R (pp. 7-8), since the 'random ordering' (apparently intended to mean that forms are relatable if any order of the rules succeeds in relating them) suggested there always allows a potentially feeding relationship to be actualized--if some rule potentially feeds another in a given derivation, then it will feed it for some order of the rules. It thus may well be that Janda's suggested proposal is more restrictive than L&R's, although this remains to be shown (it has not been demonstrated whether the latter can generate languages which the former cannot--I suspect that this is not the case). Thus, even if the random ordering proposal is rejected, this does not imply that Janda's suggestion should be.

It is also worth noting here that neither L&R nor Robinson 1980 'reject the principle of random rule-ordering', (Janda, p. 22), but rather that

they only 'disfavor' it (Janda, p. 24). L&R, for example (p. 8), 'conclude that linear ordering, insofar as it is tenable, is desirable' (my emphasis). If linear ordering should be shown not to be tenable, then random ordering, or perhaps Janda's proposal, could be tentatively adopted. It is not clear to me why L&R are only tentative in their rejection of random ordering, since Leben 1979:183 and Robinson 1977:9 take some pains to point out that UDP is not (non-inherently) global, and unrestricted globality and random ordering appear to be equivalent in generative power from the perspective of UDP. (This has not been shown to be the case, but neither has any proponent of UDP shown that it is not the case; globality has indeed gotten 'bad press' (see above), and this is presumably the reason that Leben and Robinson have been so quick to renounce it.)

Another not terribly damaging problem discussed by Janda (pp. 40-1) concerns the 'abstractness' of UDP. Although L&R (pp. 5-6) mention this issue only indirectly, it is pretty clearly a direct concern of Robinson 1977, and so merits some discussion here. The final representations arrived at in an UDP derivation do indeed appear to be 'abstract' in many cases, and UDP is thus not much different from, say, Chomsky and Halle 1968 in this respect. What is more, a less abstract rightside-up theory (e.g., one incorporating an 'alternation condition--cf. Kiparsky 1973) would undoubtedly decrease any difference in abstractness between such theories and UDP. On the other hand, I cannot agree with Janda (pp. 26, 56-8) that the constraint against absolute neutralization brought about by Robinson's revised procedure for rule application ((4) above) is not more intrinsic to UDP than Kiparsky's 1973 'alternation condition' is to standard theories; if (4) is indeed intended to replace fully conditions (2b) and (2c), as it appears to be, then this constraint does in fact follow directly from independently motivated principles of the theory. That is, this decrease in abstractness does seem to be an integral part of UPD (or at least the Robinson 1977 version).

A related point is Janda's (pp. 39-40) discussion of the 'solid body of evidence that phonological theory must countenance at least some abstractness.' First of all, it is not at all clear exactly how solid this body is (Janda refers here to so-called 'external evidence'). The discussion in Sommerstein 1977, Churma 1979:ch. 5, and Manaster-Ramer 1980 (although the latter is undoubtedly overcritical) indicates that the kinds of evidence which Janda cites must be taken with a grain of salt. What is more, a theory of UDP such as that proposed in Pollack 1977a, b, and Leben 1979, in which lexical representations are fairly abstract--more so than classical phonemic representations--would take care of at least some of this evidence. Adopting such a theory, however, does result in diachronic problems, as will be shown below (section 4).

Similarly, the fact that some phonological rules must apparently apply productively and in rightside-up fashion (pp. 51-2) need not be problematic for a theory of the type just mentioned, as long as the inputs to the rightside-up rules are not too abstract (and in Janda's example, they apparently are not), although, again, diachronic problems would result from the adoption of such a theory.

3. Some further synchronic problems for UDP.

It should be clear from the discussion given above that UDP deserves serious attention as a possible theory of phonology. Furthermore, since

even the genuine problems discussed above appear not to be totally devastating ones, it might be tempting to some to (at least tentatively) adopt UDP. I will argue in this section that this should not be done; in particular, I will argue that UDP is inadequate as a theory of phonology (and morphology) because it fails to characterize appropriately the class of possible morphophonologies of natural languges. Thus, for example, it will be maintained that there are natural languages whose morphophonology cannot be adequately characterized within the theory (i.e., that UDP is too weak). It will also be maintained that there is at least one example for which UDP can provide a straightforward characterization which does not correspond to a possible morphophonology of a natural language (UDP is too strong). If UDP in its present form(s) does in fact fail in both these respects, then this at least suggests that minor modifications of the theory will not succeed in remedying such failures: any obvious decrease in the restrictiveness of a principle of UDP (thus alleviating the 'too weak' problem) would, if anything, aggravate the problem of being too strong, and vice versa.

This is not to say that no modification of UDP could succeed. In fact, the modification given in Robinson 1977 both allows some morphophonologies that the L&R version did not and does not allow some which the latter did (thus making the two versions incommensurable with respect to generative capacity). One could legitimately question whether such a radical revision really leaves us with the 'same theory' we started with,[7] however, and I will thus not seriously consider the possibility of making such radical changes in the theory except when they have actually been proposed; to do so is an impossible task at any rate, since the number of possible such changes is infinite. Specifically, I will not consider the possibility suggested by Janda in several places of allowing exception features in UDP. I agree with Janda (p. i) that condition (2c) 'is crucial to (the spirit of) UDP', and since perhaps the principal claimed virtue of UDP is that this condition allows UPD to do away with exception features, the lack of such features is correspondingly crucial.[8]

One further issue deserves some discussion before I proceed to the task at hand, one which may appear to compromise this entire task. I will depend for the most part on specific analyses of various languages, and it might be objected that there is no assurance that these analyses are the correct ones (cf. Fn. 6). This is quite true, and perhaps unfortunate in the best of all possible worlds but in this world such a situation appears to be unavoidable. There simply are no neutral 'empirical data' which can be used to falsify UDP, or any other scientific theory (cf., for example, Kuhn 1970). Since a discussion of a large number of examples would clearly be impractical, I will limit myself to a relatively small number. I will thus depend, as any scientist must, on the assumed relevance of the 'data' discussed to the question at issue. I would expect, however, that the not at all insignificant number of examples offered, together with the existence of numerous parallel cases from other languages (specified in more detail below), in the following discussion will suffice to convince most investigators that my claims are in fact well supported.

3.1. UDP is too weak.

I will first offer several examples which indicate that UDP, in one or more of its actually proposed forms (and perhaps other versions as

well), is unable to provide a morphophonological grammar for all possible (in fact, it is claimed here, actual) natural languages. I would like to reiterate that more or less trivial loosenings of the restrictions of the theory will undoubtedly not be relevant here, since this would result in an increase in the number of languages generable by a theory which can already generate impossible languages.

3.1.1. Let us first consider an actual case in which it appears that UDP cannot (directly) relate two actually related words without, as Janda puts it (p. 20) 'global phonological rules which look farther ahead than at their own output'. As pointed out above, both Leben and Robinson seem quite opposed to allowing this kind of globality, and at any rate, since allowing such globality would only permit the generation of further languages not previously generable, in addition to those generable by the original, doing so would, if anything, aggravate the problem that UDP is already overly strong.

The case in question comes from Icelandic, and was originally discussed in Anderson 1969, 1974. L&R:8-9 discuss this example as well, but fail to recognize that the relevant words can be related only by violating condition (2c), or by making it global in the sense that rules can be undone if compatibility is thereby increased at some indeterminate future stage of the derivation. The rules involved include u-umlaut and syncope (described in section 1 above), i- umlaut, which converts a to e when followed in the next syllable by i, and the l/r rule given below, together with the required derivation, in (5).

(5)

	Word A	Word B	Morphology
LEXICAL FORMS:	$[ketill]_{ns}$	$[kötlum]_{dp}$	$[N\text{-}r]_{ns}$; $[N\text{-}um]_{dp}$
a. l+r → l+l	$[ketilr]_{ns}$	_____	_____
*b. i-umlaut	$[katilr]_{ns}$	_____	_____
c. u-umlaut	_____	$[katlum]_{dp}$	_____
d. syncope	_____	$[katilum]_{dp}$	_____

The asterisk indicates a rule which can only be undone by violating the non-global version of condition (2c). Note that the starred stage (5b) causes the vowel affected to differ from the one with which it is being compared by three distinctive features, roundness, backness and height, whereas it only differed from its mate by one feature, roundness, before i-umlaut was undone. Note further that attempting to undo u-umlaut first will not help matters, since this would lower and back the ö, thus adding another feature difference to the already existing one.

One possible way of remedying this situation would be to require that all rules be undone simultaneously, if possible, checking after each set of simultaneous applications for compatibility until no more rules can apply.[9] However, this approach would make it quite difficult to check to see if condition (2c) were being violated; all we would know after checking the output of a block of rules which resulted in a violation of (2c) is that at least one of the rules in this block was responsible

for the violation. We would thus have to continue trying all possible combinations of rules, leaving out suspected culprits, until we either found a combination which (2c) permits or had exhausted all possible combinations. This, of course, would make for a situation in which the parsing procedure would be, as L&R put it (p. 8), much less determinate. This approach, then, does not look very appealing.

Another possibility is to relate these words via a third word, and in this case one could in fact succeed in relating them via katli (dat. sg.). Since Robinson 1980:132 apparently wants to bar such a possibility for 'comparing the members of a single paradigm' (cf. the discussion in sec. 2.1 above), I will not pursue the matter further. Note also that allowing the use of this procedure in cases like this would take away the diachronic advantage of distinguishing 'leveling in paradigms and leveling across paradigms' which Robinson claims the theory to have.

It should be noted that even random ordering would not help here, as long as condition (2c) is retained. (Random ordering--or even linear ordering--would work if this condition were eliminated, but it is, as noted above, a crucial part of the theory.) The reason for this is that undoing either of the umlaut rules results in a, a vowel which is less like either ö or e than they are like each other. Thus, no matter what the order is, undoing an umlaut rule will result in a decrease in compatibility.

The procedure suggested in Robinson 1977 does in fact allow the umlauted vowels to be related, as illustrated in (6).

(6)	Word A	Word B	Morphology
LEXICAL FORMS	$[ketill]_{ns}$	$[kötlum]_{dp}$	$[N\text{-}r]_{ns}$; $[N\text{-}um]_{dp}$
a. l+r → l+l	$[ketilr]_{ns}$	———	———
b. i-umlaut	$[kVtilr]_{ns}$	———	———
	$[ketilr]_{ns}$	———	———
c. u-umlaut	———	$[kVtlum]_{dp}$	———
	———	$[ketlum]_{dp}$	———
d. syncope	———	$[ketilum]_{dp}$	———

To 'undo' i-umlaut according to procedure (4) above, we first substitute variable coefficients for those features mentioned in its output, here [-low, -back]; I indicate the result of this operation by V, no matter what features have variable coefficients (I trust that no undue confusion will result from this practice). Comparing the corresponding vowel in the other word, we find that it is [-low, -back], and so replace both variables by a minus sign, thus converting the 'archivowel' (back) to e. To undo u-umlaut, three features must be given variable coefficients, since its output would be [-low, -back, +round]. Examining the corresponding vowel in the first word, we find the specifications [-low, -back, -round], and so substitute these values for the variables, with the result being e.

But undoing rule (6a) appears to violate the conditions of this procedure, since it requires (Robinson 1977:7-8) that 'there must be evidence, from the items being compared, for the insertion of a specific feature value different from that found in a given segment...' (my emphasis; cf. also 4.iii above). Here, however, the 'evidence' which would permit the

replacement of [α lateral] by [-lateral] (i.e. taking surface l back to r) comes not from the items being compared, but from what is called for by the WFR's in question, and thus the full undoing of this rule is blocked. One might suggest that I am being over-literal in my interpretation of Robinson's intentions in this respect, and that it was just an oversight on his part that he failed to mention the possibility of using evidence from WFR's to replace the variable feature coefficients. But this is apparently not the case, since not permitting such evidence is crucial to successfully ruling out the undoing of the rules which this procedure is designed to rule out.

Let me illustrate this with one of Robinson's examples, based on the analysis of Nupe suggested in Hyman 1970. This analysis, which Robinson wishes to show is not permitted by UDP (in its corresponding upside-down form, of course), contains rules (7) and (8) below (cf. Robinson 1977:8).

$$(7)\quad \begin{bmatrix} V \\ +\text{low} \end{bmatrix} \rightarrow \begin{bmatrix} +\text{back} \\ -\text{round} \end{bmatrix}$$

$$(8)\quad [C_i \begin{bmatrix} -\text{cons} \\ +\text{syl} \\ 2\text{tone} \\ +\text{high} \\ \alpha\,\text{round} \\ \alpha\,\text{back} \end{bmatrix} - [C_i \begin{bmatrix} V \\ \alpha\,\text{round} \end{bmatrix} X]_V]_N$$

Rule (7) is a phonological rule of 'absolute neutralization' which converts ɔ and ɛ (and a, vacuously) to a, and rule (8) is a WFR which forms participles from corresponding verbs by reduplicating the initial consonant cluster and producing a mid toned high vowel with the backness and roundness features of the vowel which follows this cluster (i.e., i if it is unrounded, u if it is rounded). But when Robinson discusses the attempt to relate [tʷá] 'to trim' and [tʷūtʷā] 'trimming', which are putatively related by rule (8), he claims (p. 8) that, by virtue of rule (7), 'we may in fact substitute [α back] and [β round] on the [á]'s' of these forms. However, 'we cannot go on to insert the values [+back] and [+round], since there is no evidence for those specific feature values on those vowels', despite the fact that 'the morphological rule...makes crucial use of the feature [+round]'. These remarks can only be interpreted, as far as I can see, to mean that the required 'evidence' in Robinson's revised procedure cannot come from WFR's; if it could, then [+round], at least, could in fact be substituted, since it is crucially called for by rule (8). Thus, Robinson's revised procedure for undoing rules appears to encounter very real difficulties. They do not involve its 'excessive power,' as Janda puts it (see section 2.2 above), at least in cases like that from Icelandic just discussed, but in fact just the opposite--there are languages which clearly should be generable by the theory, but cannot be generated as long as procedure (4) is maintained. And the reason for these difficulties is not (Janda 1980:31) that this procedure would 'deprive the notion "phonological rule" of its essential content as a pairing of input and output...' (and note that there is no reason necessarily to expect the 'content' of this notion to remain unchanged in a theory which posits the radically different function for phonological rules), but rather that it would deprive phonological rules of the possibility of increasing the compatibility of two forms with respect to a given WFR whenever such an

increase results from altering a segment so that it looks more like what is called for by this WFR. This is thus a quite general problem for this procedure; the example given would not be an isolated one, and in fact this kind of problem would arise whenever, in a rightside-up treatment, a phonological rule affects an affix. Note also that making the obvious revision, so that the WFR's can also be checked to see if there is any increase in compatibility, would allow not only the l/r rule to be undone, but also some rules[10] of absolute neutralization, the banning of which was the motivation for the revised procedure. It thus seems clear that Robinson's revision is no improvement over 'standard' UDP, and that the revised revision just suggested violates the spirit of Robinson's proposal, since it does not rule out absolute neutralization rules. What is more, both of these revised versions of UDP are still too strong (i.e., can generate impossible languages), as will be shown in section 3.2. This example thus appears to be quite damaging for UDP in any currently proposed version, as well as in several suggested further revised versions. It should be noted that there is no reason to believe that this example is an isolated one (although I cannot at present suggest any further examples of this type), since, as Janda (pp. 24-5) has pointed out, rightside-up derivations need not always decrease the compatibility of morphologically related words, and it is such a decrease which causes undoing a rule to increase compatibility.

3.1.2. The next example involves the interaction of a rule of reduplication and two phonological rules in Tagalog. This example has been fairly widely discussed (cf. especially Wilbur 1973, Anderson 1975, Carrier 1975 and Herbert 1977), and not all investigators agree as to the formulation of the phonological rules involved. The differences are not crucial to this discussion, however, and I adopt here Herbert's quite convincing analysis. The phonological rules involved, then, will be one of nasal assimilation, which assimilates a nasal to a following obstruent with respect to point of articulation, and one of obstruent deletion, which deletes an obstruent when preceded by a nasal. The interesting thing about these processes is that in rightside-up theories the morphological rule of reduplication (which copies the first vowel of the stem and the consonant which immediately precedes it) appears to apply to the output of the phonological rules, so that /maŋ+bigay+REDUP/ 'give' (fut.), for example, becomes first /mamigay+REDUP/, and finally [mamimigay]. Consider now what an upside-down derivation involving these rules, and relating mamimigay and bigay, would look like, given in (9).

(9)

	Word A	Word B	Morphology
LEXICAL FORMS:	[bigay]	[mamimigay]	$[maŋ+C_iV_i+[C_iV_iX]]_{V\ fut}$
a. Obstruent del.	_____	[mambimigay]	_____
b. Nasal assim.	_____	[maŋbimigay]	_____

The WFR in question forms the future of verbs by prefixing maŋ and copying the first CV of the verb stem. Note that, even after both of the relevant phonological rules are undone,[11] the two words cannot be related, and there would be no other rules which could convert the last m back to a b, given a system which is even close to traditional rightside-up accounts.

This sort of problem will arise, it seems to me, whenever a morphological rule gives the appearance of applying to the output of a phonological rule in a rightside-up treatment (cf. note 12 below for examples from the literature). The only way I can see of getting around this problem is to allow morphological rules to be undone (perhaps, following Aronoff 1976, only in certain places in the block of phonological rules) as well as phonological rules. This would entail a considerable modification of the theory, since currently there is a radical difference between phonological rules and WFR's in UDP, with the latter being 'redundancy rules' in the sense of Jackendoff 1975. Since the latter do not apply generatively, they cannot be (generatively) undone either.[12] It should also be noted that such a revision would occasionally lead to stems which never occur in isolation on the surface, as in Janda's (p. 13) example *pi- from pious/piety, and the status of such a representation in UDP is quite questionable, as it is in rightside-up theories which entail such representations (cf., for example, Halle 1973, Jackendoff 1975, Aronoff 1976). What is more, as Janda suggests in several places, allowing such a possibility (at least together with other revisions such as getting rid of condition (2c), etc.) could make for a theory which is essentially a notational variant of standard theories. Thus, examples of this type appear to present significant difficulties for the theory.

3.1.3. UDP will also encounter difficulties whenever what appears to be a single rule applies both word-internally and across word boundaries in a rightside-up treatment. Since L&R state (p. 2) that 'only fully regular morphological and phonological processes will apply in the formation of new words from existing words, or in the derivation of external sandhi variants', there is no explicit provision in their theory or any elaboration of the theory by Leben or Robinson that I know of (but cf. Pollack 1977a for an extensive discussion of this issue), for the treatment of external sandhi phenomena when the processes involved are not fully regular.[13] It is not clear to me why they have restricted the operation of external sandhi rules in the way that they have, but even in the cases which they have provided for, the theory appears to encounter serious difficulties. The problem arises when, as noted above, what would appear to be a single rule in rightside-up theories applies in both internal and external sandhi environments, since (if it is 'fully regular') it will apply rightside-up in the external sandhi cases, since all words are entered in the lexicon in their 'surface representations' (p. 1). Note that L&R apparently do not want to allow for rightside-up derivations of existing words even if the processes involved are fully regular, since such processes are said to apply in the formation of <u>new</u> words (see the above quotation). We thus seem to be forced by the theory to treat Russian voicing assimilation, for example, as two separate phenomena,[14] one when it occurs word-internally, and another when it occurs across word boundaries. Thus, while rightside-up theories would analyze the phonetic forms in (10) (data from Sullivan 1975 and Halle 1959) by positing the corresponding underlying representations in (10) and deriving the phonetic forms by a rule of voicing assimilation, UDP would have to enter the first two forms in the lexicon in their phonetic representations and undo the voicing assimilation rule in (10b) to relate the two stems.

(10) a. [podojtí] 'approach' (perfective): /pod+ojtí/
b. [potxodít] 'approach' (imperfective): /pod+xodít/
c. [mokl,i] 'was (he) getting wet': /mok#l,i/
d. [mogbi] 'were (he) getting wet': /mok#bi/

In the case of the last two forms, however, it is not possible to enter them in the lexicon in their phonetic forms, since they are made up of two separate words, and therefore each requires two separate lexical entries (one for each word), as they would in rightside-up theories, with the voicing assimilation rule applying rightside-up in such cases to derive the phonetic form, again as in rightside-up theories. Thus, although the 'same rule' is involved in both types of examples, the fact that it applies in 'normal' rightside-up fashion in one of the cases, but has to be undone in the other, indicates that the analysis is making the claim that there are really two different phenomena going on here, one which is handled in the traditional way, and one which is treated by undoing traditional rules. Such cases do not appear to be at all rare. English flapping, for example, seems to be another obvious cancidate for an example of this type, since it too occurs both word-internally and across word boundaries.

Pollack 1977a, 1977b has also noticed this problem, and has proposed that lexical representations in UDP should be made considerably more abstract than L&R propose, so that they correspond to the lexical representations of 'natural phonology' (cf. Stampe 1973, Donegan and Stampe 1979).[15] If this approach is adopted, then the cases just discussed can be treated just as in rightside-up theories, since the lexical representations would then be the same as in such theories, and the phonetic forms would be derived by the live 'natural processes' of voicing assimilation in Russian and flapping in English. It is only those alternations which cannot be characterized in terms of natural processes (i.e., are due to the operation of rules in Stampe's sense), Pollack proposes, which are to be handled in upside-down fashion. But, while this approach would allow these phenomena to be treated in the unitary manner which seems to be required, its good points are not unaccompanied by bad points as far as UDP is concerned, as will be argued below (section 4).

Furthermore, even adopting the Pollack/Leben revision cannot take care of all problems of this type. Rather, any case which requires a lexical representation which is more abstract than those allowed for by natural phonology still would appear to present a problem for UDP. The particular case to be discussed here, French liaison, is especially troublesome in that it does not seem possible to give an analysis similar to that given for the Russian case. Here the standard rightside-up rule must apparently apply upside-down to produce the sandhi variants, and it must do so in the same way that rules apply in rightside-up theories--generating a phonetic form which differs from its lexical representation--a possibility which is not envisioned in any treatment of UDP which I am aware of, and one which appears to be quite undesirable as long as rightside-up rules can also apply productively in rightside-up fashion as well. It should be noted that this kind of productive rightside-up application of a rule appears to be necessary whenever a deletion or insertion rule, rather than a feature-changing rule, is involved, so this problem is not a peculiarity of this particular example.

Consider now the forms in (11):

(11) petit ami [pətitami] /pətit#ami/
petit garçon [pətigarsõ] /pətit#garsõ/
petits amis [pətizami] /pətit+z#ami+z/
petits garcons [pətigarsõ] /pətit+z#garsõ+z/
petite amie [pətitami] /pətit+ə#ami+ə/
petite fille [pətitfiy] /pətit+ə#fiy+ə/
petites amies [pətitsami] /pətit+ə+z#ami+ə+z/
petites filles [pətitfiy] /pətit+ə+z#fiy+ə+z/

Standard rightside-up analyses (e.g., Schane 1968) posit lexical representations of the type given on the right, and the corresponding phonetic forms are derived by a rule which, roughly, deletes consonants preconsonantally and prepausally, together with one which deletes schwa in environments which need not concern us here. But this kind of lexical representation would presumably not be allowed in natural phonology, since consonant deletion is no longer a live process in French (cf. sac [sak], etc.), and so UDP could not adopt the standard analysis even if lexical representations were allowed to be as abstract as they are in natural phonology.

It is not entirely clear to me what the UDP analysis of these data would be, but it does seem clear that there will have to be at least two different ways of handling this apparently unitary phenomenon. If we accept L&R's proposal (p. 1) that 'lexical representations of words are expressed in their surface-phonetic isolation forms',[16] then petit and petits would be lexicalized as /pəti/, while petite and petites would be /pətit/. Relating the masculine and feminine isolation forms (and the preconsonantal forms) is a relatively straightforward affair in UDP: the standard rule relating masculine and feminine forms of adjectives, given in (12), would be employed, together with the previously mentioned phonological rules, as in (13).

(12) Word-Formation Rule: ə-Attachment

$$[\text{Amasc } -\text{ə}]_{\text{Afem}}$$

(13)

	Word A	Word B	Morphology
LEXICAL FORMS	$[\text{pəti}]_{\text{Amasc}}$	$[\text{pətit}]_{\text{Afem}}$	Rule 12
a. ə-deletion	———	$[\text{pətitə}]_{\text{Afem}}$	———
b. C-deletion	$[\text{pətit}]_{\text{Amasc}}$	$[\text{pətitə}]_{\text{Afem}}$	———

The plural isolation forms can be accounted for in much the same manner.

However, nothing said so far accounts for the prevocalic variants, which also must have the isolation forms as their lexical representation. It is these cases which apparently require the productive upside-down use of the standard deletion rule, for we must somehow obtain phonetic [pətit] from lexical /pəti/, for example, in prevocalic environments. It

should be noted that, in order to determine which consonant to insert ([t] for singular petit, [n] for bon, [z] for mauvais and all plurals, etc.), the rule presumably requires transderivational or translexical power (cf. Lakoff 1973) in that the final consonant present in the feminine singular form will (usually) be the one to appear in the masculine prevocalic variants; otherwise, unless each masculine form is lexically marked for the consonant to be inserted, it is simply impossible to tell which consonant to insert. Condition (2c) is, as noted above (section 2.2), inherently translexical in nature (as is procedure (4)), but while this may be quite natural when two words are being compared to determine whether or not they are morphologically related, it seems much less clear that translexical power should be allowed in deriving surface forms from lexical forms.

Let us now briefly examine the possible revision of UDP mentioned in Fn. 16, that of listing the variants in question in the lexicon. Under this approach, petit would be entered as something like /pəti~pətit/ in the lexicon, or perhaps, following Hudson 1974, 1980, as /pəti{t}/, where the braces indicate that the t is not always realized in the phonetic form. Similarly, petits would be /pəti{z}/, and petites /pətit{s}/. Petite, at least on the basis of the data given above, shows no variation, and would be simply /pətit/, although in a full treatment it would contain a final {ə}, since a final schwa is present before so-called 'aspirate h' words. In addition, rules would be required to derive the phonetic forms from these; I will assume for the purposes of discussion that the consonant deletion rule mentioned above will do the job, although I am not sure how technically feasible this actually is. Whatever the actual form of the rules, however, this approach seems to entail a quite counterintuitive claim, namely that the lack of a t in the lexical representation of petits and the presence of a {t} in that of petit are two quite unrelated phenomena. There is no t in the former because there is none in any of its phonetic variants, and braces are required in the latter because one variant has a t and the other does not. Similarly, petite has a simple t in its lexical representation because it always shows up in the pronunciation. All of these phonetic facts have a straightforward explanation in standard rightside-up theories, namely that in all of these cases the morpheme /pətit/ is involved. Whenever the final t in this morpheme precedes another consonant or a pause, it is deleted; there is never a t in the pronunciation of petits because the underlying t is always followed by another consonant, the plural morpheme /z/, and it only sometimes shows up in petit because whether or not it is followed by a consonant depends on the initial segment of the next word. But the version of UDP under discussion does not relate the total lack of t in the phonetic variants of petits to the invariant presence of the consonantal plural morpheme, despite the fact that it does claim that in other cases the presence or absence of t is due to the nature of the following segment. As long as one feels, as I do, that the real reason for the lack of t in phonetic forms is the same in both cases (i.e., the presence of a following consonant), then the inability of this version of UDP to express this reason can only be considered a significant failing of the theory.

Thus, unless lexical representations are made even more abstract than those in natural phonology, UDP appears to be forced to treat cases such as French liaison as at least two separate phenomena.[17] Such a degree of

abstractness would further weaken any claims of the theory about explanatory power with respect to sound change (cf. section 4 below).

3.1.4. The final example which indicates that UDP is too weak comes from Kasem. There is considerable disagreement about the optimal treatment of the Kasem facts, as presented in Callow 1965, 1968 (cf. Chomsky and Halle 1968, Howard 1969, 1970, Anderson 1969, 1974, Phelps 1975, 1979, Goyvaerts 1978, and Halle 1978); I will base my discussion on Anderson's analysis, since it is this analysis which is cited by L&R (p. 9) as being 'particularly troublesome' with respect to rule ordering, but most of the issues raised will be relevant to all of the published analyses of Kasem nominals that I know of. I will argue that the Kasem data are 'troublesome' with respect to more than just rule ordering.

Consider the singular and plural forms for the word for 'sheep', pia (sg.) and pe (pl.). Anderson analyzes these forms as being underlyingly /pia+a/ and /pia+i/, respectively, where -a is the singular marker and -i the plural marker for this class of nominals. The derivation of the phonetic forms involves three rules, one of metathesis which interchanges the first two of a sequence of three vowels, one of truncation which deletes one of a sequence of identical unrounded vowels, and one of contraction which converts, for example, /ai/ to /e/. The rightside-up derivations which Anderson posits are given in (14).

(14) a.

/pia+a/	Input
pia	Truncation

b.

/pia+i/	Input
paii	Metathesis
pai	Truncation
pe	Contraction

The ordering relationships in the above derivations are determined according to the principles of Anderson's 'local ordering', but are of no relevance to the point considered here (see Anderson 1974 for further discussion). Let us now attempt to relate the surface forms using Anderson's rules in an upside-down derivation, as in (15).

(15)

	Word A	Word B	Morphology
LEXICAL FORMS:	$[pi+a]_{sg}$	$[pe]_{pl}$	$[N\text{-}a]_{sg}$; $[n\text{-}i]_{pl}$
a. Contraction	_____	$[pa+i]_{pl}$	_____
*b. Truncation	$[pia+a]_{sg}$	$[pai+i]_{pl}$	_____
c. Metathesis	_____	$[pia+i]_{pl}$	_____

It is somewhat unclear whether or not condition (2c) will permit the undoing of Contraction in (15a), since, while undoing it makes Word B look like the plural of something with respect to the plural WFR given in (15), it does not make it look like the plural of Word A. It is thus not at all certain that undoing this rule would 'increase the compatibility of forms A and B with respect to [this] Word-formation Rule', as is required by condition (2c).

This problem is not unique to (the upside-down version of) Anderson's analysis in fact, it remains in all of the analyses of Kasem cited above, and it is perhaps worthwhile to digress a moment to take a brief look at the analysis which undoubtedly has the least in common with the others, that of Phelps 1975, in this respect. The relevant rules are one of "Truncation", which deletes the second of a sequence of two vowels agreeing in backness and roundness (p. 313), and a rather unusual rule (cf. Halle 1978:181) called "Vowel Height Exchange", which among many other things, converts ea to ia (cf. Phelps 1975:314 and the revised version in Phelps 1979:37). These rules apply (in this order) to convert her underlying singular /pe+a/ to the required surface pia, via the latter rule, and the plural /pe+i/ to pe, by applying the former. The corresponding upside-down derivation is given in (16).

(16)

	Word A	Word B	Morphology
LEXICAL FORMS	$[pi+a]_{sg}$	$[pe]_{pl}$	$[N-a]_{sg}$; $[N-i]_{pl}$
a. V Height Exchange	$[pe+a]_{sg}$	———	———
b. Truncation	———	$[pe+i]_{pl}$	———

The undoing of (16a) is problematic in much the same way that the undoing of (15a) was. Here, the (putative) stems are made to look more like each other, in fact identical. But it is not easy to see that the words in question are thereby made more compatible with respect to the relevant WFR's, since Word B (still) does not look like the plural of anything. That is, as far as the WRF's are concerned, the two words are (still) totally incompatible. An obvious way of resolving this problem in a way which seems favorable to UDP is to define 'compatibility' as in (17):

(17) The compatibility of Words A and B with respect to Word-Formation Rules $R_1, R_2, \ldots$, is increased by undoing a given phonological rule if

i. the putative stems, or an affix called for by more than one of the R_is, in the two words are thereby made to look more similar, in that there is an increase in feature specifications shared by corresponding segments, at least one of which was present prior to the undoing of the rule,[18] or

ii. one of the words is made to look more like something called for by one of the WFR's, i.e., is made to look more like a member of the morphological category called for by this WFR, in the sense specified in i.

Such a definition would indeed permit the undoing of the rules under discussion, as well as that mentioned in note 11. It is not clear to me whether this is in fact what was intended by condition (2c), since 'compatibility' is nowhere explicitly defined by the proponents of UDP. If it is adopted, of course, it would, if anything, only exaggerate the difficulties faced by UDP in virtue of the fact that it is already overly strong.

But let us return to the question of derivation (15). Undoing Truncation in (15b) is required to obtain the eventual match, but disallowed even if condition (2c) is interpreted in accordance with (17): the words already look exactly like a singular and a plural, respectively, and so (17ii) is not met, and the stems are clearly not made more compatible by (17ii) (if anything, they are made less compatible, since there are now two pairs of incompatible segments, whereas before there was only one). What is more, it seems to me, undoing Truncation will not make the stems any more compatible under any obvious reading of this expression, and so is blocked by any reasonable interpretation of condition (2c). Note that no other linear order will help here, and, moreover, that even applying the rules in different orders for the words will not allow these words to be related.[19] Thus, pia and pe cannot be related in this theory, despite their seemingly clear morphological relationship, given an Anderson-type analysis, or, it will be noted here (without justification, due to space limitations), in any of the analyses mentioned above, aside from those of Phelps, which, as hinted at earlier, are rather suspicious. It can thus be seen that the Kasem facts, unless they are subjected to substantial further reanalysis, present quite severe difficulties for UDP.[20]

3.2. UDP is too strong. Since the possibility of using exception features is at least potentially a genuine drawback of standard theories, in that theories which permit their use may be able to describe impossible languages and thus are themselves overly strong, UDP's ability to do without such features makes it appear to be a quite attractive alternative. However, if it can be shown that UDP has only transferred the (putatively) objectionable power of exception features to some other device of the theory, then it can be seen that it is not nearly as attractive in this respect as it might at first appear to be. This, together with the fact that UDP in its currently proposed forms cannot generate some languages which it should, thus requiring some modification in the direction of being able to generate languages which it now cannot (cf. the discussion at the beginning of section 3), makes it doubly important to give careful consideration to this issue.

I will argue here that UDP has in fact merely transferred at least some of the power of exception features to some other aspect of this theory. It may even have more objectionable power than some standard theories, since such theories, even with exception features, appear to be unable to handle the following example, given a constraint on abstractness such as that of Kiparsky 1973. The reason this is so is that what would be exceptions to a rule in rightside-up theories correspond to the non-necessity of undoing a rule in UDP. Whether there are exceptions to a given rule in a rightside-up account, or how many exceptions it has, is thus totally irrelevant from the standpoint of UDP. That is, a rightside-up rule which applies in 99.99% of the cases in which its structural description is met is not distinguished in any way which I can see from one that applies only 0.01% of the time. It is this characteristic of the theory which allows it to generate impossible languages and thus makes it too strong.

Let us consider now an example which demonstrates this. Within UPD, pairs such as father/paternal, mother/maternity, etc., which most linguists would undoubtedly maintain are at best only distantly related synchronically,[21]

can be related 'without resorting to ad-hoc means' (L&R:4), just as caprice and capricious can. Consider the rules given in (18). Rule (18a) relates not only the [ð] of father and the [t] of paternal (via /θ/ by (18b)), but also singular/plural pairs such as elf/elves, path/paths, etc., and so presumably cannot be ruled out by the evaluation measure.[22]

(18) a. $\begin{bmatrix}+\text{cont}\\+\text{obstr}\end{bmatrix} \rightarrow [+\text{voi}]/\text{V}\_\_[+\text{voi}]$

b. $\begin{bmatrix}+\text{obstr}\\-\text{cont}\end{bmatrix} \rightarrow \begin{bmatrix}+\text{cont}\\-\text{voi}\end{bmatrix}$

c. $n \rightarrow \emptyset / \begin{bmatrix}+\text{cons}\\+\text{son}\end{bmatrix} \_\_\#$

(18b) is involved in at least ten alternations, six [t]/[ð] alternations (mother/maternal/maternity and the corresponding alternations with father and brother, again in concert with (18a)), and two [b]/[f] and [f]/[p] pairs of alternations in the father and brother sets,[23] so it too would apparently be sanctioned by the evaluation measure. It is perhaps worth mentioning here that the fact that evidence for (18b) comes only from the items in question which are in fact claimed not to be morphologically related, does not prevent the evaluation measure from sanctioning this rule in UDP, since it does not 'know' this. All that is available to the evaluation measure is the fact that such pairs are semantically related, and the ten 'alternations' in question can be 'accounted for' by this rule. (18c) will handle hymn/hymnal, column/columnar, damn/damnation, etc., as well as the six [n]/∅ 'alternations' involved here. We will also need the clearly independently motivated rules assigning stress and that of vowel reduction (cf., for example, Chomsky and Halle 1968). The derivation relating father and paternal using these five rules, all well-motivated from the standpoint of UDP, is given in (19).

(19)

	Word A	Word B	Morphology
LEXICAL FORMS	[fáðər]$_N$	[pətʌ́rnəl]$_A$	[N-əl]$_A$
a. Rule (18a)	[fáθər]$_N$	———	———
b. Rule (18b)	[pátər]$_N$	———	———
c. Rule (18c)	[pátərn]$_N$	———	———
d. Stress	[patərn]$_N$	[pətʌrnəl]$_A$	———
e. V Reduct.	[patʌrn]$_N$	[patʌrnəl]$_A$	

Note that in this derivation none of the stages violates condition (2c), so that father and paternal can be readily related, requiring only two more steps than the sane/sanity derivation, and the same number of steps as that relating caprice and capricious. Evidently, in this theory the father/paternal-type pairs are at least no more distantly related than caprice and capricious. This is in spite of the clearly extravagant nature of (at least) rule (18b) and the lack of such extravagance in L&R's rules.

One further point deserves mention in connection with this discussion. It may have been noticed that rule (18b) is context-free, i.e., it converts all stops to voiceless fricatives, regardless of their phonological context. Clearly, this rule has an immense number of exceptions to it, or at least it would have in a rightside-up theory. But, as noted above, this is simply irrelevant from the standpoint of UDP. Leben himself has noticed (Leben 1979:199) that there is nothing in the version of UDP given in L&R to prevent rules from being 'absurdly general', but suggests that the 'recoverability principle' he proposes there 'limits the generality of the rules'. I cannot see why this would be so; in the other cases discussed by Leben (pp. 191-8) for which there is a recoverability problem, this problem is solved not by eliminating or altering some rule which is causing the problem, but by changes in the forms themselves which permit their relationship to become more recoverable with respect to the existing rules. I thus fail to see why Leben feels that this principle has the effect he claims it does with respect to 'absurdly general' rules. What the principle actually seems to predict in the case at hand is that rule (18b) is a part of the grammar of English (for the reasons given above), and that, due to the recoverability problems caused by this rule, forms will change so as to increase their recoverability with respect to it. (The forms under discussion would not be the ones predicted to change, it should be noted, since here there is no recoverability problem in Leben's sense--see section 4 for discussion). Thus, the fact that 'exceptions' to rightside-up rules 'don't count' in UDP represents a serious flaw in the theory. That is, even if the possibility in UDP of having 'incomplete' rules (McCawley 1979) is for other reasons an attractive one, there are also problems caused by this possibility, and these are not gotten around by Leben's 'recoverability principle'.

It should be noted that the procedure suggested in Robinson 1977 (cf. 4 above) will also allow the successful relating of these words, since in this case the 'evidence...for the insertion of a specific feature value' (Robinson 1977:7) does indeed come 'from the items being compared', and not from what is called for by a WFR, as was the case in the Icelandic example discussed above (section 3.1.1). Thus, not only does this procedure make it impossible to relate, for example, the Icelandic forms, but it also allows father and paternal to be just as easily related as in the original version of the theory. It should be noted that this relating involves the use of a rule of absolute neutralization, (18b), and that it is thus not the case that Robinson's revised procedure would (p. 9) 'rule out abstract analyses'; while it may well rule out more or less plausible such analyses (although I am not certain that it would), it fails to disallow implausible ones--analyses which no rightside-up account would have seriously considered. The reason is, again, the 'exceptions' simply do not matter in UDP.

Finally, note that the possibility of having rules like (18b) also would appear to make a characterization of rule naturalness or markedness very difficult for UDP, since the rules would lack the phonological contexts which often influence naturalness in rightside-up theories.

4. Some further diachronic considerations.

As noted above, UDP is claimed to have 'diachronic advantages' over rightside-up theories (L&R:11). I would like to turn not to a brief examination of this claim. It is argued (L&R:14) that UDP offers an explanation for the fact that 'sound change affects only surface items' because of 'the purely derivative nature of non-phonetic representations' in the theory, whereas 'in standard theory it must be stated as an extrinsic

constraint on change (cf. King 1973)'. As mentioned above (section 3.1.3), however, this advantage disappears from versions of the theory, such as those proposed by Pollack and Leben, in which lexical representations are not 'surface items'. Notice that lexical representations in the Pollack/Leben model can be quite abstract (i.e., relatively different from the phonetic representations). In the Russian example, of course, they would be more abstract than the 'classical phonemic' representations, as Halle 1959 has pointed out, and they undoubtedly would be in the English case as well. But if UDP claims to explain the putative fact that sound change affects only phonetic representations (i.e., L&R's 'surface items') by having such representations in the lexicon (and, presumably, claiming that phonological change affects only lexical representations), it cannot allow for such a revision. Once any degree of abstractness is permitted in lexical representations, moreover, UDP is in precisely the same position as any rightside-up theory as far as permitted phonological changes are concerned; the putative diachronic explanatoriness can be maintained only by maintaining fully concrete lexical representations. Thus, even if a position such as the one suggested (p. 2) by L&R that some less abstract, but still not fully phonetic, level is the level of lexical representation should be adopted, the diachronic consequences of the theory will not be as claimed by L&R. Precisely analogous problems would arise concerning their claim (p. 19) that their theory makes it possible 'to make room for lexical gradualness' in diachronic change if the Pollack/Leben proposal (or any proposal with 'abstract' lexical representations) is adopted. Thus either this model must be given up (thus leading to the synchronic problems mentioned in section 3.1.3) or these claims of the theory about diachronic explanatoriness cannot be maintained.

Let us now turn to a brief examination of the diachronic implications of Leben's 'recoverability principle' (cf. section 3.2). This principle, together with UDP's blindness toward exceptions to rightside-up rules, and the evaluation measure, makes some startling (and obviously incorrect) claims about change. For the evaluation measure, unless perhaps it is revised to reflect 'recoverability' somehow, would dictate that every rule in UDP should be context-free, because of the features 'saved' by getting rid of environmental restrictions. This, of course, would result in wholesale recoverability problems, since 'the established chain of rules' (Leben 1979:198) would be broken frequently with many cases of these 'absurdly general' rules not requiring application. The corresponding diachronic prediction is wholesale changes in pronunciation to alleviate these problems.

It thus would appear that a closer look at Leben's 'recoverability principle' is in order. Unfortunately, I am not fully confident that I understand what Leben intends in this respect, since he nowhere explicitly defines this term, and the relevant discussion is not a model of clarity. It is therefore open to question whether the definition given in (20), which is pieced together from various places in Leben 1979, accurately reflects his intentions.

(20) Recoverability Principle: If

(i) 'from looking at a given surface form' one cannot 'immediately tell' (p. 194) what the surface form of the corresponding 'base word' should be (i.e.,

a segment in a 'derived word' potentially corresponds to more than one segment in 'a base word' (p. 197), and

(ii) there is no 'orderly chain of segments that are successively tried in searching for an appropriate... word' (p. 196) by virtue of 'a chain of successfully applied steps' (p. 197) in a UDP derivation,

then the difficulty of recovering the morphological relationship between the two forms is significantly great, and the pronunciation of some forms is predicted to change in order to alleviate this difficulty.

Particularly puzzling to me are statements which seem to indicate that only one of (20i) or (20ii) need be satisfied in order to create a recoverability problem. Thus, for example, Leben (p. 197) outlines 'the principle suggested here, that recovery is hindered when there is more than one path that can be followed in taking a segment in a derived word back to a representation compatible with the corresponding segment in a base word...', while he notes on the next page that the nonnecessity for undoing Vowel Shift in relating caprice and capricious 'breaks the established chain of rules, pointing to a recovery problem under the principle proposed above.' This suggests that satisfying (20ii) will suffice to cause a recoverability problem. He even seems to imply (e.g., p. 197) that he feels that the two conjuncts of (20) are synonymous. Nevertheless, since in his discussion (pp. 195-6) of an English example, he indicates that he feels that (20i) is satisfied (but (20ii) is not), and that yet there is an 'absence of any sign that...pressure [for change in this case] is being felt', it seems most likely that Leben intended a conjunction, and not a disjunction, of (20i) and (20ii).

Assuming that this is in fact what he intended, two questions immediately present themselves. First of all, why is it that recovery is 'hindered' when a 'derived word' potentially corresponds to more than one 'base word' but not vice versa? It seems clear that the 'vice versa' situation is not all rare--one cannot, for example, tell immediately from looking at the German surface form [bunt] whether the corresponding derived word should be [buntə] or [bundə]--and I can see no reason why such a situation should be any less problematic for recoverability than that described in (20i). It could be that not allowing for recoverability problems to result from this kind of thing represents a simple oversight on Leben's part, and he might well have intended that the 'recoverability principle' be extended to cover situations like this. But if he did, then it is difficult to see that the 'recovery problem' was 'rectified' (p. 194) by the Polish changes discussed by Leben, and so let us take a brief look at these changes. In Old Polish, there were pairs such as radość 'joy'/ rado[śn]y 'joyful', zawiść 'grudge'/zawi[stn]y 'begrudging', post 'fast' /po[śn]y 'lenten', and głо[s] 'voice'/głо[śn]y 'loud' (cf. pp. 189-90, 193). Leben attributes the changes from rado[śn]y to rado[sn]y and from po[śn]y to po[stn]y to the fact that they (p. 197) 'aided recovery of the segments in question'. This is probably true, since there is now only one 'base form' which corresponds to 'derived forms' in [śn], while in Old Polish there were three.[24] But notice that this change had no

effect on what would also have been a recoverability problem under this interpretation--there are still two kinds of derived forms which correspond to base forms in ść, just as there were in Old Polish (although one of these derived forms is different than it was in Old Polish). It should also be noted that this change, while it has eliminated the recoverability problem with respect to derived forms in [śn], has also introduced one with respect to derived forms in [stn], since there are now two corresponding base forms instead of the single base form in Old Polish. Note further that (20ii) is still not satisfied after the change, since C Drop (cf. note 24) must be blocked by condition (2c), and the 'chain of successfully applied steps' will thus be broken, in the relating of głos and głoś]y (assuming, in accord with condition (2a), that the C Drop rule precedes the one responsible for the s/ś alternation in a UDP derivation). That is, the changes in question had a rather minimal effect on recoverability, and, what is more, entail a rather substantial alteration in the grammar of Polish.

Leben never gives a clear picture of what the grammar is like at either stage, but judging from Gussmann's account, there would have to be a rule reordering and the loss of another rule,[25] both of which appear to carry over to the UDP account. In Gussmann's account, some lexical items must acquire a diacritic marking them as exceptions to C Drop (p. 302) as well, but this will of course not carry over to the UDP account. This would appear to be a rather extreme reaction to the recoverability problem, given the meager amount of resulting improvement in terms of recoverability (see above). This is especially true from the standpoint of a theory like UDP, where speakers memorize pronunciations and are only rarely called upon to recognize morphological relationships. In fact, from the standpoint of such a theory, it is not all clear why speakers should be particularly bothered by recoverability problems, certainly not enough to make the drastic changes in the grammar required in the Polish case. Moreover, one can very well question the relevance of a state of affairs like that described in (20i) in an account which (Leben, p. 198) 'makes crucial use of the characteristic...of sometimes factoring complex alternations into a sequence of simple ones...'; if this possibility is so important, then how can the ability to 'immediately tell' (i.e., without a sequence of steps) what kind of 'complex alternation' there is also be of critical importance? Why go through the sequence of rules if you already know the answer to your question?

The second question mentioned above concerns the fact that all that is predicted by the principle is that some forms will change. Nothing at all is said about which forms will change, or about the direction in which change will proceed. Without some refinement in the predictive power of the theory, the Polish changes, which certainly seem rather unlikely from the standpoint of standard theories (at least, changes like that from rado[śn]y to rado[sn]y), would appear to be regarded as rather unlikely from the standpoint of Leben's revised version of UDP as well.[26] There was, after all, another way of alleviating the recoverability problem, namely doing away with allomorphy completely by changing rado[śn]y to rado[śćn]y, and indeed, something precisely analogous happened in the case of po[śn]y. Moreover, this is not the only change which would seem to be viewed as more likely than the actual change from the standpoint of the theory. A change to rado[stn]y, for example, would appear to give

all the recoverability advantages of the actual change, and also decrease allomorphy in some sense, since there would be a consonant cluster in the derived form corresponding to the stem-final cluster in the base form. (In addition, this would alleviate the problem of nonpredictability of derived forms from a given base form, if the recoverability principle is taken to cover such cases--see the discussion above.) That is, without some such refinement, the recoverability principle cannot be taken to offer a very good explanation of the changes in question.

The diachronic objections raised by Janda and discussed and elaborated on above (section 2.1) are also relevant here, but I see no need to discuss them further.[27]

5. Psycholinguistic issues.

L&R:3-4 suggest that psycholinguistic evidence might provide further support for their theory. It seems to me, however, there is a fair amount of evidence of this type which would create problems for the theory.

First of all, it is well known that children tend to 'over-generalize', apparently using rules when they should not from the standpoint of the adult system, even when they know the correct adult form (cf., for example, McNeill 1970). This would appear to be very good evidence that they are in fact using word-formation rules and phonological rules productively. Thus, unless we are willing to accept that adult grammars are radically different from child grammars, in that in the case of the latter most rules apply rightside-up and productively, while for the former they usually apply upside-down (if at all) and surface pronunciations are for the most part memorized (recall that few rules can meet L&R's criterion of being 'fully regular'--cf. Fn. 13), it would seem that adults too should use productively and in rightside-up fashion processes which are not fully regular in L&R's sense. There is some more direct evidence which points to the same sort of conclusion as well, such as the classic Berko study (cf., again, Fn. 13). It is worth pointing out that some of the rightside-up uses of rules suggested by Berko's study require at best marginally productive rules in anybody's theory, such as those which relate sing, sang and sung; this fact would appear to be quite troublesome for the theory no matter how 'fully regular' is interpreted.

A further kind of direct evidence (of at least a semi-psycholinguistic nature) for the productive rightside-up use of WFR's which are not fully regular is that from the study of speech errors (cf., for example, Fromkin 1971). Slips such as groupment for grouping and concludement for conclusion (p. 45) can only be explained, as far as I can see, as the productive use of the WFR which forms nouns from verbs by adding -ment.[28] Yet, this rule is not 'fully regular' in L&R's sense, due to its semantic idiosyncrasies (cf. government), as well as, at least for some speaker's, phonological ones (no [n] in government). This WFR thus could apparently not be used by L&R's criterion.

One final bit of semi-psycholinguistic evidence should be mentioned here. It concerns the apparent ease (noted in Hetzron 1975:870) with which speakers can tell whether or not they have previously heard a word which is the result of a process of derivational morphology (or one of radically irregular inflectional morphology, I would add), as contrasted with one which results from a regular inflectional process, in which case speakers are much less clear about whether they have heard it before.

This seems to suggest a different kind of lexical storage in the two kinds of cases. While in the case of the first type, it might well be possible to maintain that words are stored as wholes (i.e., as L&R:1 put it, 'the surface representations of words'), this hardly seems a legitimate possibility in the latter type of case. To maintain otherwise would appear to be equivalent to saying that speakers have memorized the (surface representations of) words in both cases, but that speakers have conscious knowledge of these memorized forms only in cases of the former type, and in cases of monomorphemic words, but not in the other cases. Even if one were willing to go along with this (in my view) absurd position, it seems to be clearly incumbent upon an adherent of such a position to provide an explanation for the difference in question. None has ever been offered, to the best of my knowledge. The implications of this as far as UDP is concerned are that, at least in cases of the second type, it apparently cannot be held that (L&R:1) 'the lexicon...excludes all but the surface representations of words', or even some more abstract representations of words; it must contain at least some bound morphemes.

Let us now briefly consider a different psycholinguistic issue, that of language acquisition, from the standpoint of UDP. In order to acquire competence in the phonological and morphological aspects of the language being acquired, a child must least at least the following: (1) lexical representations of words; (2) morphological rules; and (3) phonological rules. In addition, the child must also learn the order in which the phonological rules apply if extrinsic ordering is permitted, and, if the Pollack/Leben model is taken somewhat loosely (so that the Stampean 'natural processes' are not considered to be innate), natural processes. For the purposes of this discussion, I will assume that the acquisition of all of the above is unproblematic except in the case of the phonological rules (but see the preceding few paragraphs for some discussion of morphological rules). These rules, judging from the examples provided by Leben and Robinson in their work, are given in rightside-up form, and therefore must presumably be learned in such a form by the child if UDP is to have anything at all to say about language acquisition. Yet, at least in the case of rules which are not 'fully regular,' they are never used in the way which the form of the rule suggests (i.e., rightside-up). Such rules are always undone--never does a rule of this type apply to the 'input' to produce the 'output'. This leads one to question how a child could acquire a rule which will never be used in the form in which it is acquired. And, although it might not be easy to imagine how a child could acquire the rules required in standard theories (they are, after all rather complex and abstract), it seems even more difficult to understand how rules could be acquired if these rules are never used as such, as appears to be the case if UDP is adopted.[30]

At this point, it might be suggested that UDP should not be committed to the rightside-up rules of standard theories, but rather that the phonological rules in a given analysis need have nothing to do with the corresponding rules in standard theories. That is, UDP should be positing rules which are 'upside-down' only from the standpoint of standard theories, but are actually in the form which phonological rules 'should' take (i.e., are rightside-up from their own perspective); the rules given in current treatments are only for the purposes of exposition (cf. Leben 1979), and the rules in a fully worked-out UDP might take a rather different form.

If this suggestion is adopted, however, it seems that at least two new problems will be created. First, there is the problem that some rules must apparently be allowed to apply productively in rightside-up fashion, and rewriting the rules in a form which makes them look more acquirable does not appear to allow for this possibility. Moreover, such a revision of the form which rules take seems to make it quite difficult to impose any well-motivated set of constraints on the form which phonological rules may take, at least not without making reference to the form which they may take in standard theories (there would be no necessity, for example, for rules to be phonetically plausible, and in fact they would typically be just the opposite, perhaps even context-free). The lack of such constraints would subject UDP to the same criticisms which Leben 1979 raises against Natural Generative Phonology; there would be no conceivable rule, no matter how extraordinary, which is not viewed as a potential phonological rule of some natural language. No matter which alternative is chosen, then, UDP appears to be in an uncomfortable position as far as language acquisition is concerned.

6. Conclusion.

In this paper, I have argued that the theory of UDP as represented in various versions proposed in the literature, as well as in conceivable alternative versions, creates at least as many difficulties as it alleviates with respect to 'standard', rightside-up theories of phonology and morphology. In particular, I have argued (cf. section 3 above) that UDP is faced with extreme problems from a synchronic point of view, in that it incorrectly characterizes the class of possible natural languages; what is more, the characterization provided is incorrect in both of the possible respects--the theory fails to provide adequate descriptions for possible (and actual) languages, and allows for the straightforward description of impossible ones--which suggests strongly that no minor modification of the theory can fully remedy this situation. I have also argued (section 4) that, at least in versions of the theory which can alleviate some of these synchronic problems, UDP is in much the same situation as rightside-up theories with respect to phonological change, i.e., that UDP has no 'diachronic advantages' over other theories, and that a version incorporating Leben's 'recoverability principle' in fact has diachronic disadvantages. Finally (section 5), I have argued, the characterization of the grammar given by UDP is implausible on psycholinguistic grounds. Thus, even though the previous criticisms of UDP considered in section 2 were found not to be terribly forceful on the whole, there are a number of genuine problems for the theory, at least some of which would appear to be insurmountable by any revision of the theory which retains the 'crucial' condition (2c). Since it is not at all clear that any other currently proposed theory of phonology/morphology is fully adequate, we will probably have to continue our search for an appropriate such theory. The results of this paper can be of some aid in directing this search, in that we know where not to look; it seems clear that such a theory will not be, in the sense intended by Leben and Robinson, 'upside-down'.

Footnotes

*This paper is an extensively revised and expanded version of a paper presented at the 1977 Summer meeting of the Linguistic Society of America. I would like to thank P. de Zeeuw, P. Gathercole, V. Gathercole, R. Janda, B. Joseph, W. Leben, J. McCawley, and A. Zwicky for their helpful comments on that version and on another preliminary version of this paper, as well as discussion of some of the general issues involved.

[1]Janda's definition undoubtedly needs some refinement. As it stands, it implies, quite counterintuitively, that undoing a deletion rule (any deletion rule) on both forms being considered always increases their compatibility. Thus, for example, if there is a WFR which adds a suffix X to a stem to form some other morphological category, and a phonological deletion rule, then the compatibility of the forms Y and Y + X would be increased by undoing the deletion rule according to this definition, even though they already matched with respect to the WFR. Such considerations will have some relevance later on (cf. section 3.1.4); I will postpone further discussion until then.

[2]Leben (personal communication) has informed me that this interpretation is in fact what was intended. It is not clear, however, that such an interpretation would not allow for derivations which Robinson 1977 wants to rule out as too abstract, e.g., in cases where an absolute neutralization rule affects (in a rightside-up treatment) an affix and the underlying segments subject to this rule have an effect on segments in the stem. For further discussion of this question, see section 3.1.1.

[3]In Janda's discussion, he does not require that the words in question be semantically related, although he considers this possibility in a footnote (pp. 58-9), and he never considers the possibility of checking only those morphological rules suggested by the meanings of the words involved. But speakers clearly know, for example, that left is a past tense form and therefore presumably the result of a past tense formation rule, and so there is no reason not to build corresponding kinds of information into the parsing procedure. The situation in this respect is thus not nearly as bad as Janda's discussion implies, although the amount of work which must go into showing that depart and left are not related, even with the semantic restrictions suggested here (see below), seems clearly disturbing from the standpoint of UDP.

[4]Janda indicates (p. 43) that he feels that such a change is one which UDP 'should perform,' but apparently has not recognized that the alteration proposed above would in fact perform such a change, although he also states somewhat puzzlingly that all rules of all types' 'should be undone...by reversing the arrows in their respective rightside-up versions.' As a result, he comes up with a quite unwieldy revision of condition (2b). His revision, together with an apparent misinterpretation of L&R's requirements for 'matching', leads Janda to further irrelevant criticism (p. 45) of this condition; it is not 'the purpose of an upside-down derivation...to arrive at individual segments...that are identical...', but rather (L&R:3) to arrive at forms which are 'non-distinct' (presumably in the sense of Chomsky and Halle 1968:336). (It should be noted in this regard, however,

that Robinson 1977, 1980 does in fact require that segments eventually be identical (and fully specified) in order to 'match'.) Since Leben in his singly authored papers never indicates that he subscribes to a similar interpretation, it could very well be that there is some disagreement between Leben and Robinson on this count. Even so, it is not at all clear that Robinson's requirement for matching is subject to Janda's criticisms on this matter, since his 1977 paper gives a significantly different procedure for undoing rules (cf. procedure 4 above).

[5]This statement follows from the mathematics of infinite sets. For general mathematical discussion of this point, see, for example, Wilder 1965; for an application of this result to a comparison of theories of rule ordering, cf. Churma 1980c. Wilbur 1975 also contains relevant discussion of the non-inherent nature of the evils of globality.

[6]The situation is not quite as simple as this discussion might make it appear. It is always possible to contest a given analysis and, thus, the claim that this analysis corresponds to a possible natural language; and it is likewise possible to maintain that a claimed impossible language is in fact a possible one. Such a state of affairs appears to be not at all restricted to issues of this nature, or even to linguistics; I have argued elsewhere (cf. Churma 1979, 1980a) that the ultimate determinant of a given scientist's acceptance of most theoretical claims is his or her (degree of) belief in the truth of the premises of the argument which leads to the conclusion in question.

[7]Such a question is undoubtedly behind the difficulty which Janda 1980:29 has in seeing how the proposal of Robinson 1977 'could be considered to be "undoing" a rule', and behind his dissatisfaction with it. Janda never comes to grips with this theory (whatever it is) on its own terms, however, and whether or not the modified theory is still UDP has no bearing on the status of its claims about the class of possible morphophonologies. I will argue below that this theory, too, is unsatisfactory in this respect.

[8]The issue which Janda raises in several places (e.g., p. 53) of whether a version of UDP which incorporates exception features is 'a notational variant of' standard rightside-up theories is thus not a terribly interesting one. Given his beliefs about the crucial nature of condition (2c), it is somewhat puzzling why he finds it so.

[9]This is essentially the proposal which Koutsoudas, Sanders, and Noll 1974 (hereafter KSN) have made concerning the application of rules in rightside-up phonologies; i.e., rules apply whenever their structural descriptions are met. It would also give much the same results as the theory of rule application presented in Robinson 1980, although the latter could not handle cases like the one under discussion. It is worth pointing out in this respect that Robinson's theory, or the simultaneous theory just outlined, would also require something analogous to KSN's principle of 'proper inclusion precedence' to handle cases of mutual bleeding and what I refer to (Churma 1977) as transfusing-type relations (i.e., cases

in which the order of the rules has empirical consequence, but where the relation between the two rules is of neither the feeding nor bleeding type).

[10]It appears that in the Nape example discussed here, the two forms cannot in fact be related via rule (7) by this modification of Robinson's procedure, since there is 'evidence for' [+round] in rule (8), but none for the required [+back]. But this has nothing to do with the fact that (7) is a rule of absolute neutralization, and it seems that such a modified procedure could handle the German example of Bach and Harms 1970 discussed by Robinson.

[11]It is not clear that they can be undone, since (9a), while it does put back a b into Word B, does not appear to put it into a position which 'corresponds' to that of the b in Word A. Furthermore, undoing this rule destroys the match which previously existed with respect to the reduplication part of the WFR (i.e., it looked like there was reduplication before the undoing of (9a), but not after). It is therefore difficult to tell whether or not condition (2c) would be violated here. Janda is thus quite correct (p. 4) that the term 'compatibility' requires further clarification, although not for the reasons he suggests (cf. section 1 above; for further discussion of the question at issue, see section 3.1.4 below).

[12]Despite this, Leben (personal communication) has suggested that all reduplication rules should in fact be undone (presumably because they look a lot more like phonological rules than do other WFR's). In fact, he allows a Hausa reduplication rule to be undone (cf. Leben 1977a:431, 432, 438). But the Hausa rule is not a WFR, as the formulation given by Leben (p. 429), and repeated here as (i), indicates.

$$\text{(i)}\quad \underset{1\ 2\ 3}{[X(C)C]_R} - VC - V \rightarrow \underset{1\ 2\ 3}{[X(C)C]_R} - VC - C - VC - V$$
$$\qquad\qquad\qquad\quad 4 \quad 5 \qquad\qquad\quad 4 \quad 3 \quad 4 \quad 5$$

There is no mention of any morphological function in this rule, and, as far as I can tell, its only function is to make things difficult for Hausa children. The two cases are thus quite different. And, of course, it is not the reduplicative nature of the rule in question that is the issue--any WFR whose relationship to the phonological rules was of this type would entail precisely similar problems (cf. Anderson 1975:48-50, 56 for two such examples of non-reduplication rules from Danish and Rotuman).

[13]It is not clear to me exactly what is intended by the term 'fully regular'. If taken literally (i.e., meaning 'exceptionless'), it would seem to entail that English plural formation, for example, could not be used 'in the formation of new words from existing words' because of pairs such as child/children, mouse/mice, etc. This seems clearly not to be the case (cf. Berko 1958). Note further that it is apparently not possible in this theory to make regularity a matter of degree as far as directionality is concerned, since productive (phonological) rules will apply rightside-up and nonproductive rules will apply upside-down, thus not allowing for any middle ground.

[14]There is a good deal of evidence that almost all linguists who have written on the subject feel that Russian voicing assimilation is in fact a single phenomenon, i.e., that the minor premise of the argument given by Halle 1959 is true (for justification, cf. Churma 1980b). Considerable argument would thus appear to be in order if it is felt that there is more than one phenomenon involved here.

[15]Leben 1979 has apparently independently come to an essentially identical conclusion about the nature of lexical representations, although not for these reasons.

[16]If we do accept this, then it would appear that the theory has no way of even talking about sound changes which affect the alternating parts of non-isolation external sandhi variants. They would thus presumably be forced into listing every occurring 'surface-phonetic' form of every word in the lexicon in order to retain their putative explanation of the nature of sound change. Further discussion of the possibility of listing external sandhi variants in the lexicon is given below.

[17]This criticism would apparently apply to all theories which require a level of lexical representation which is less abstract than that required here, such as that of natural phonology or of Natural Generative Phonology (cf. Donegan and Stampe 1979 and Hooper 1976, and the references cited there).

[18]The qualification that one of the segments which undergoes the increase in shared feature specifications must be present prior to undoing the rule is intended to remedy the problem mentioned in Fn. 1.

[19]Note also that the principles for rule application proposed in Robinson 1980 do not determine an order for contraction and truncation, since this is a transfusing order (cf. Fn. 9), and so does not fit into the feeding-bleeding taxonomy.

[20]Robinson's revised procedure for undoing rules (cf. 4 above) would of course be even worse off, since it would straightforwardly disallow the undoing of (15a) and (16a).

[21]Not everybody would; Lightner, for example, would probably find them just as closely related synchronically as he does nation and pregnant (cf. Lightner 1975:617). Whether or not two forms are in fact synchronically related does not seem to be the kind of question that one can give rational arguments about, and so I will not attempt to do so. A Lightnerian would thus probably find this discussion irrelevant, or perhaps even supportive of the theory, just as I would find a discussion of the 'problem' of how to relate nation and pregnant in UDP or any other theory rather strange. I direct this discussion only toward those who agree that such forms as father and paternal should not be straightforwardly relatable phonologically in a synchronic grammar of English.

[22]Note that any semantically related pair, such as depart/left, could be related (by the past tense-formation rule) if we did not appeal to the evaluation measure to rule out the 'phonological' rules needed to relate the forms in question. (No exception features would be needed if we did allow these rules, however.) Note also that there would appear to be nothing any more exceptional about the singular/plural pairs with this voicing alternation than the [s]/[z] alternation in the plural morpheme in this theory. That is, if the ordinary allomorph of the English plural morpheme is taken to be underlyingly /z/, so that the relevant word-formation rule is something like $[N\text{-}z]_{Npl}$, then the theory appears to make no distinction in complexity between alternations such as cat/cats and bus/buses, on the one hand, and leaf/leaves on the other. The latter would require rule (18a) to be undone, while the former would necessitate the undoing of a rule devoicing obstruents after voiceless segments and one inserting schwa between sibilants, respectively. The fact that there is no such alternation in the great majority of forms where there is the potential for one seems to be simply irrelevant as far as UDP is concerned, despite the rather extreme counterintuitiveness of these implications of the theory.

[23]Note that this rule relates the p of paternal to the f of father, but the b of brother to the f of fraternity, so that the input to the rightside-up rule is part of the adjective form in the first case, but part of the noun form in the second. This, again, is quite counterintuitive, but irrelevant from the point of view of the theory, as far as I can tell.

[24]There is a complication not immediately apparent which should be pointed out here. Old Polish post and po[śn]y cannot be directly related using the Leben/Gussmann rules (Leben's discussion is based on that of Gussmann 1976). The reason is that, while the rule of "C Drop', which deletes a coronal consonant when it is between (among other things) s and n (Leben, p. 188), can be undone on po[śn]y to give something like po[śCn]y, the rule of palatal assimilation (which converts, e.g., s to ś before a palatal) cannot be undone since its structural description is not satisfied by a following segment which is unspecified for palatality (the 'C' would be specified only [+coronal]). Robinson's revised procedure (4 above) would also fail here, since comparison of the variable feature specifications inserted in undoing 'C Drop' with those in post would yield po[śtn]y, and structural description of palatal assimilation would still not be met. It thus may well be that UDP cannot relate these forms in Old Polish and that the theory fails solely on this count, with the recoverability principle, or the lack thereof, being quite beside the point. My knowledge of Old Polish is not sufficient to allow me to state that the forms in question cannot be related indirectly via a third form, although if the behavior of po[śn]y parallels that of rado[śn]y, it appears that they cannot, since in the latter case (p. 194) 'the noun stem within the adjective invariably appeared as radoś-' in Old Polish, and we would thus be faced with exactly the same situation with any member of the case paradigm.

[25]I refer here to what Gussmann calls (p. 301) the 'depalatalization rule which must depalatalize spirants before nonpalatal consonant clusters.' Gussmann may have intended that this function be performed by a generalization of a rule he calls (p. 291) 'anterior depalatalization', since he does not show a separate step corresponding to the former rule in his Old Polish derivations. If this is so, then the change in question requires a rule 'degeneralization' instead of a rule loss, as well as synchronic iterative application of (anterior) depalatalization. It is worth pointing out in this respect that if Old Polish had a rule of 'palatal assimilation' (Gussmann, p. 292) which not only cause 'dental spirants [to] assimilate to the place of articulation of the following palatal', but also performed the function of the depalatalization rule (i.e., if it made coronal spirants agree in palatality of a following consonant), then this additional rule (or generalization of one) is not necessary, as long as palatal assimilation can be applied to the output of anterior depalatalization. However, since the former is crucially ordered before the latter in Gussmann's account (although it is reordered so as to follow it in modern Polish), this would entail an ordering paradox. Whether or not Gussmann's analysis of Old Polish is the optimal one is thus certainly an open question, and it is unfortunate that Leben bases so much of his discussion of recoverability on it.

[26]This is not unique to UDP; it would also be the case for any rightside-up theory, such as that of Gussmann, which invokes a similarly vague recoverability principle.

[27]It should also be noted that, as I have argued elsewhere (Churma MS), a version of UDP which does not allow for 'rule inversion' (Vennemann 1972) as a mechanism of change--and Leben at least apparently does not want to (cf. Leben 1974, 1979)--cannot provide a reasonable account of certain changes in the Chadic language Kanakuru. This problem is not unique to UDP, however; it seems that the Kanakuru changes would be quite problematic for any theory which rejects rule inversion.

[28]I cannot see why Fromkin feels that it is 'possible' (let alone 'highly probable') that the latter slip involves 'a blend of concluding and conclusion': where did the -ment come from? If she only means by this that this slip does not provide terribly strong evidence for the productive application of a phonological rule converting d to z in rendering conclusion, however, I would have to agree.

[29]Such considerations would appear to indicate problems for any rightside-up theory of which I am aware, as well. What appears to be needed, at least as far as the nature of lexical representations is concerned, is some sort of combination of UDP (or some similar theory) and standard theories.

It should also be noted in this respect that the suggested 'line' between morphological processes need not be precisely as indicated here for the general point to hold; as long as there is some such division, there will be a corresponding problem for UDP (and standard theories, as long as no morphologically complex lexical entries are permitted by such theories).

[30]Leben (personal communication) has questioned the relevance of these considerations on the grounds that 'it is hard to see what difference rule directionality would make to an account' of acquisition when what is acquired from the standpoint of UDP is taken to be 'the ability to abstract away from permissible allomorphy in perceiving morphological relationships'. I suppose that the considerations in question would indeed be irrelevant from such a point of view, but then it is hard to see what facts about acquisition could possibly be relevant to such a vague characterization of this process. That is, given only this characterization of language acquisition, UDP simply fails to attempt to solve 'the problem to which the linguist addresses himself....to account for the child's construction of a grammar...' (Chomsky and Halle 1968:331).

References

Anderson, Stephen R. (1969). West Scandinavian Vowel Systems and the Ordering of Phonological Rules. Bloomington, IN: Indiana University Linguistics Club.

Anderson, Stephen R. (1974). The Organization of Phonology. New York: Academic Press.

Anderson, Stephen R. (1975). On the interaction of phonological rules of various types. Journal of Linguistics 11:39-63.

Aronoff, Mark. (1976). Word Formation in Generative Grammar. Cambridge, MA: MIT Press.

Bach, Emmon and Robert D. King. (1970). Umlaut in modern German. Glossa 4:3-21.

Baker, C. L. and Michael K. Brame. (1972). 'Global rules': a rejoinder. Language 48:51-75.

Berko [Gleason], Jean. (1958). The child's learning of English morphology. Word 14:150-177. [Reprinted in Child Language: A Book of Readings, ed. by A. Bar-Adon and W. F. Leopold, 153-167. Englewood Cliffs, N.J.: Prentice Hall.]

Callow, John C. (1965). Kasem nominals--a study in analyses. Journal of West African Languages 2.1:29-36.

Callow, John C. (1968). A hierarchical study of neutralization in Kasem. Journal of Linguistics 4:33-45.

Carrier, Jill. (1975). Reduplication in Tagalog. MS., MIT.

Chomsky, Noam and Morris Halle. (1968). The Sound Pattern of English. New York: Harper and Row.

Churma, Donald G. (1977). On 'mutually non-affecting' rules. Winter LSA Meeting, Chicago, IL.

Churma, Donald G. (1979). Arguments from External Evidence in Phonology. Ohio State University Ph.D. Dissertation.

Churma, Donald G. (1980a). Diachronic evidence for synchronic analyses in phonology. This volume.

Churma, Donald G. (1980b). A further remark on the 'Hallean syllogism'. This volume.

Churma, Donald G. (1980c). Rule inversion in Chadic: a closer look. MS., Ohio State University

Churma, Donald G. (1980d). We must argue about rule ordering. Linguistic Analysis 6:41-51.

Dinnsen, Daniel A., ed. (1979). Current Approaches to Phonological Theory. Bloomington, IN: Indiana University Press.

Donegan, Patricia Jane and David Stampe. (1979). The study of natural phonology. In Dinnsen 1979.

Goyvaerts, Didier L. (1978). Aspects of post-SPE Phonology. Ghent: E. Story-Scientia.

Gussmann, Edmund. (1976). Recoverable derivations and phonological change. Lingua 40:281-303.

Halle, Morris. (1959). The Sound Pattern of Russian. The Hague: Mouton.

Halle, Morris. (1973). Prolegomena to a theory of word formation. Linguistic Inquiry 4:3-16.

Halle, Morris. (1978). Further thoughts on Kasem nominals. Linguistic Analysis 4:167-85.

Hetzron, Robert. (1975). Where the grammar fails. Language 51:859-72.

Hill, Jane H. (1970). A peeking rule in Cupeño. Linguistic Inquiry 1: 534-39.

Herbert, Robert K. (1977). Reconstructing phonetic processes in Proto-Austronesian. Austronesian Symposium, Honolulu, HI.

Hooper, Joan B. (1976). An Introduction to Natural Generative Phonology. New York: Academic Press.

Howard, Irwin. (1969). Kasem nominals revisited. Working Papers in Linguistics 1.10:63-99, University of Hawaii.

Howard, Irwin. (1970). Kasem nominals and the ordering of phonological rules. Working Papers in Linguistics 2.3:113-29, University of Hawaii.

Hudson, Grover. (1974). The representation of non-productive alternations. Historical Linguistics I, ed. by John M. Anderson and Charles Jones, 203-29. Amsterdam: North-Holland.

Hudson, Grover. (1980). Automatic alternations in non-transformational phonology. Language 56:94-125.

Hyman, Larry M. (1970). How concrete is phonology? Language 46:58-76.

Jackendoff, Ray. (1975). Morphological and semantic regularities in the lexicon. Language 51:639-71.

Janda, Richard D. (1977). 'Upside-down' phonology: regenerative or degenerative? Winter LSA Meeting, Chicago, IL.

Janda, Richard D. (1980). 'Upside-down' phonology: regenerative or degenerative? Bloomington, IN: Indiana University Linguistics Club.

King, Robert D. (1973). Rule insertion. Language 49:551-78.

Kiparsky, Paul. (1973). Phonological representations. Three Dimensions of Linguistic Theory, ed. by Osamu Fujimura, 1-136. Tokyo: TEC Company, Ltd.

Koutsoudas, Andreas, Gerald Sanders, and Craig Noll. (1974). The application of phonological rules. Language 50:1-28.

Kuhn, Thomas S. (1970). The Structure of Scientific Revolutions, second ed., Chicago, IL: University of Chicago Press.

Lakoff, George (1973). Some thoughts on transderivational constraints. Papers in Linguistics in Honor of Henry and Renée Kahane, ed. by B. B. Kachru, 442-52. Urbana, IL: University of Illinois Press.

Langendoen, D. Terence. (1975). Acceptable conclusions from unacceptable ambiguity. Testing Linguistic Hypotheses, ed. by D. Cohen and J. Wirth, 117-27. Washington, D.C.: Hemisphere Publishing Company.

Leben, William R. (1977a). Doubling and reduplication in Hausa plurals. Linguistic Studies Offered to Joseph Greenberg, ed. by Alphonse Juilland, 419-39. Saratoga, CA: Anma Libri.

Leben, William R. (1977b). On the interpretive function of phonological rules. Phonologica 1976, ed. by Wolfgang U. Dressler and Oskar G. Pfeiffer, 21-7. Innsbruck: Innsbrucker Beiträge zur Sprachwissenschaft.

Leben, William R. (1977c). Parsing Hausa plurals. Papers in Chadic inguistics, ed. by Paul Newman and Roxanna Ma Newman, 89-106. Leiden: Afrika-Studiecentrum.

Leben, William R. (1979). The phonological component as a parsing device. In Dinnsen 1979.

Leben, William R. and Orrin W. Robinson. (1977). 'Upside-down' phonology. Language 53:1-20.

Lightner, T. M. (1975). The role of derivational morphology in generative grammar. Language 51:617-38.

Manaster-Ramer, Alexis. (1980). On psychological reality. CLS 16, Chicago, IL.

McCawley, James D. (1979). Comment. In Dinnsen 1979.

McNeill, David. (1970). The Acquisition of Language. New York: Harper and Row.

Newton, Brian E. (1971). Ordering paradoxes in phonology. Journal of Linguistics 7:31-53.

Phelps, Elaine. (1975). Simplicity criteria in generative phonology: Kasem nominals. Linguistic Analysis 1:297-332.

Phelps, Elaine. (1979). Abstractness and rule ordering in Kasem: a refutation of Halle's maximizing principle. Linguistic Analysis 5:29-69.

Pollack, Jay M. (1977a). Lexical features in phonology. Ohio State University Ph.D. Dissertation.

Pollack, Jay M. (1977b). 'Upside-down' phonology and natural processes. CLS Book of Squibs, ed. by Samuel E. Fox, et al., 74-75. Chicago, IL: Chicago Linguistic Society.

Robinson, Orrin W. (1977). Abstractness in a phonological parsing model. Winter LSA Meeting, Chicago, IL.

Robinson, Orrin W. (1980). Rule ordering in a parsing model of phonology. Phonology in the 1980's, ed. Didier L. Goyvaerts, 119-39. Ghent: E. Story-Scientia.

Schane, Sanford A. (1968). French phonology and morphology. Cambridge, MA: MIT Press.

Sommerstein, Alan H. (1977). Modern phonology. Baltimore: University Park Press.

Stampe, David L. (1973). A Dissertation on Natural Phonology. University of Chicago Dissertation.

Sullivan, William J. (1975). Alternation, transformation, realization, and stratification revisited. The First LACUS Forum, ed. by Adam Makkai and Valerie Becker Makkai, 472-522. Columbia, SC: Hornbeam Press.

Wilbur, Ronnie Bring. (1973). The phonology of reduplication. Bloomington, IN: Indiana University Linguistics Club.

Wilbur, Ronnie Bring. (1975). Morphology and the rule ordering controversy. Berkeley Linguistic Society 1.460-74.

Wilder, Raymond L. (1965). Introduction to the Foundations of Mathematics, second edition. New York: John Wiley & Sons.